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ARC LIGHTING.

IT strikes one as being not a little strange that arc lighting for the interiors of public or other large buildings should have made such little progress in England, for it is very evident that in many places where incandescent lamps are now employed for indoor illumination, a much better effect might be produced by means of a few well arranged arcs, but the difficulty has always been to find a lamp which would meet the exact requirements. Some six or seven years ago it was thought that the "lampe soleil" was the precise thing, and so far as absolute steadiness and beauty of light was concerned the effect was beyond adverse criticism, but its practical working left much to be desired. It was not automatic in striking the arc, although attempts, which met with only partial success, were early made to render it so; it was not available for continuous current apparatus, and it could only be worked in series with alternators. The subject received much attention in the columns of the ELECTRICAL REVIEW at the time it was first brought before the public notice, and we believed that in the "Sun" lamp there was a really admirable method of electric lighting which only required developing and perfecting to make it the arc light of the day *par excellence.* Its luminosity waned, however, and after a time it apparently became hidden in the clouds of obscurity, to be soon entirely forgotten except by a few who had faith that the "Sun" which had for the time being set, would one day again shed its rays around with all its old lustre, and we are glad to see that there are signs which indicate that this will shortly be the case. There is little doubt that at the time when the "Sun" lamp made its debût, its entry into the electric lighting world was premature, even had it been more perfect in its action, but since then many changes have taken place. Steady improvements have almost revolutionised dynamo machinery; transformers, which in the early days of the "lampe soleil" existed only in the nature of experimental apparatus, are being largely employed, and now that the lamp can be automatically lighted and be run in series or parallel with either continuous or alternating current generators, it would appear that the day has arrived for its re-entry into the electric lighting arena amid a perfect blaze of triumph, for there is doubtless a great field for the improved lamp in such places as picture galleries, lecture halls, concert rooms, theatres, music halls, clubs, factories, &c.

With regard to the light itself, it has been admitted by such authorities on colour as Sir Frederick Leighton, Mr. Alma Tadema, Sir Coutts Lindsay, and others, to be the best of artificial lights, the brilliant, warm, mellow, and steady softness of the light produced contrasting strikingly with the cold, blue, hard, and piercing light of an ordinary arc.

We learn that a syndicate is in course of formation with the object of placing the improved lamp in the market, and we really believe that if the apparatus be thoroughly well reintroduced to the public it will revive great interest in arc lighting and create a demand for big lights. Attempts have already been successfully made to run up to high candle-power with incandescent lamps, and it is no uncommon thing to see them reaching 500 candle-power, but, of course, it will be readily understood that this result can only be obtained at considerable expense as compared with the arc system.

We shall hail with satisfaction the advent of the improved "Sun" lamp, which, if properly financed, and if the business accruing thereto be intelligently and honestly conducted, should prove a great success.

THE MAXIM-WESTON COMPANY.

THE "revelations" made at the meeting on Wednesday of the Maxim-Weston Company compel us to wonder how it is possible such a course of irregularities as are imputed to Mr. Hugh Watt could be pursued for so long a time without detection. Some of the allegations are of so strong a character—indeed, they even point to criminality—that an action at law seems inevitable. Two suits are pending already between Mr. Hugh Watt and the Maxim-Weston Company. Both are brought by the late chairman: one to procure the compulsory winding up of the company, the other the rectification of the register. Mr. Watt, according to the statement of the present directors, has since resigning his seat on the board sold all his shares with the exception of 24; but the directors refused to transfer them for the reason that they considered that Mr. Watt was indebted to the company to a considerable extent, instead of the company being, as he alleged, his debtor. Whilst he held his shares the company had a lien upon them in regard to any moneys due from him, and therefore they refused to transfer them to other persons. Mr. Watt's second action is to enforce the transfer. But in addition to these suits, surely another will be entered upon. We can scarcely imagine that any man can rest quiet under such accusations as are made against Mr. Watt, and as the latter is proverbially fond of resorting to law, we may almost count upon the institution of a third action, this time for libel, against the directors of the Maxim-Weston Company.

Our own opinion of the manner in which Mr. Watt has conducted the Maxim-Weston Company during the past five years has been pretty freely expressed from time to time; but this has had reference only to what appeared to the outside public. Manipulation of the accounts and questionable methods of dealing with the stock—unquestionable, perhaps, would be the more correct term to employ, for, if true, they are unquestionably dishonest—formed no part of our indictment. We should shrink—we shrink now—from believing that a man occupying the position which Mr. Watt holds would resort to tactics which can only be stigmatised as unmitigatedly fraudulent. Mr. Watt has issued an answer to the directors' report and the report of the accountants, but it was not a very strong document, and certainly will not be accepted as a complete refutation of the more specific charges made in the chairman's speech to the shareholders on Wednesday. That the shareholders are strongly incensed at the conduct of their late chairman was evident from the indignant cries of "Shame" at the meeting, and the repeated counsel to the board to prosecute. A resolution was passed authorising the directors to take such action as might seem necessary to recover from Mr. Watt certain sums of money which they considered was due from him to the company; but it was understood that an action of this kind would only be resorted to as a means of self defence. Still, it is pretty clear that the Law Courts will be the scene of further acts in the Watt and Maxim-Weston drama.

FROM time to time the question of telegraphic communication between the West Indian Islands, and between that group and the American continent, affords certain West Indian journals a favourable occasion to condemn the policy of granting a subsidy to the West India and Panama Telegraph Company. The complaints brought forward against this company are: that the news telegrams are meagre and inaccurate; that the rates levied by the company are extortionate; and that the service is inefficient. The company on the other hand assert that the traffic is insufficient, and the subsidies too small for the maintaining of a service which at the same time would satisfy the public and prove remunerative to the company. We are informed that the attempt made by the West India and Panama Telegraph Company to prevent the opening to the public of the cables recently laid by the French Syndicate, known, we believe, as the Antilles Cable Company, was founded on the concession granted to them in may 1868. We are furthur told that with the exception of Cuba, the new cables touch at places hitherto unconnected by telegraph, namely, Hayti, Santo Domingo, Curaçao and Venezuela. A plea adduced in favour of the new enterprise is that its founders have bestowed the advantages of telegraphic communication to places which but for them would have remained unconnected with the outer world. It is argued that as the older companies would not take upon themselves the risks attending upon an extension of their systems, it ill behoves them to act the part of a dog in the manger. We admire the refreshing simplicity of an argument wherein telegraph companies are expected to attain a higher moral tone and a greater delicacy of sentiment than other business combinations.

IN the latest issue of *La Lumière Electrique* Mons. de Fonvielle contributes a criticism on the late meeting of the British Association at Bath, which, by the admirers of that institution, must be considered on the whole as distinctly favourable. He is inclined, however, to be gently sarcastic when he refers to the inhabitants of Bath. He says: "If the meeting had been held in a populous and industrial town as last year in Manchester, and as next year in Newcastle, electricity would have sufficed, with the assistance of the phonograph, to have made the Bath meeting one of the best attended the British Association could boast of; but when one considers the wonders which the successes of Franklin and Volta have accomplished, the inhabitants of Bath are somewhat akin to the natives of the South Sea Islands when they witnessed the approach of Cook's vessels. Among all the prodigies of contemporary science the only one known to the natives of Bath is the electric telegraph. Their principal industry is to offer sanctuary to old officers of the Indian army who come to nurse their rheumatisms where the veterans of the Roman Legions also sought relief. The success of Prof. Ayrton's lecture on the transport of power to a distance was a revelation to the municipal councillors who, becoming aware of the presence of the Avon, have understood for the first time that its majestic tide can give them, almost without expense, a light rivalling that of the day."

WE cannot conceive anything more sickening than the description of Mr. Harold Brown's experiments on the effects produced in dogs by continuous and alternating currents. These experiments, carried out at Columbia College, were effected in the presence of a large number of electricians, on whose insensible ears the howlings of the wretched animals created seemingly no feelings of pity. After all the cruelty inflicted no useful result appears to have been obtained. One miserable dog submitted to successive applications (during 18 minutes) of electromotive forces of respectively 1,000, 1,100 1,200 1,300 and 1,400 volts survived, and was magnanimously adopted by his persecutor.

WE suppose the Electric Traction Company, Limited, will shortly be conveying passengers on the Canning Town-Plaistow tramway in electrically propelled cars. Six cars are to be employed, and a bold offer has been made to work the line for the North Metropolitan Tramway Company at the cost of 4½d. per car mile, the present price on this particular section being 6d. with horses as the motive power. The average expense per car-mile throughout the North Metropolitan system is said to be 5d., and the saving of even ½d. per mile would mean an increased profit of something like £10,000 to £12,000 per annum. At the end of the contract period the Tramway Company has the option of taking over the electric system, but whether this is done will, of course, mainly depend upon the working cost not exceding 6d. per car-mile. We sincerely trust that some reliable method of arriving at every item of expenditure will be employed, and that no loophole will be left for the financial critic to expose any doubtful figures, for we cannot help thinking that with a practical system and sound management the Traction Company's estimate should be commercially successful. Nothing should be left undone to make this trial one which may be looked upon as a safe guide for future operations, for much will depend on the result. It would, indeed, be a red letter day in the history of electric traction to know that the Canning Town-Plaistow line, which is but little over a mile in length, had been regularly worked for six months with only six cars at a total cost of only 4½d. per car-mile, with, perhaps, something in hand at the conclusion of the time.

THERE are said to be 150,000 carbons burned daily in the electric lights used in the United States, of which 100,000 are manufactured in Cleveland, Ohio. Six years ago all the carbons burned in that country were made in a single room in Boston. Now there are twenty carbon furnaces in Cleveland alone. The capacity of an ordinary furnace is 45,000 carbons.

A SIMPLE and efficient electrical device for taking soundings, and giving an alarm on board a vessel when it approaches shoal water, has been patented by two gentlemen residing in the city of Mexico. The body of the sounder is a metal cylinder, with its bottom wood-lined and chambered to receive a central socket in which is inserted the lower end of a tube made of insulating material, preferably glass, closed at its upper and lower ends by discs of conducting material, the tube being about half filled with mercury. Brackets on the inner surface of the cylinder support a plate having a metallic collar enclosing an elastic packing which covers and seals the top end of the tube, the rest of the cylinder being filled with shot, to hold the sounder perpendicularly in the water. A conical head is fitted to the sounder, and a cable passing through it carrying two electrical conductors is attached to the top plate within the cylinder, one of the conductors being connected with a disc closing the top of the mercury tube, while the other conductor is connected with a similar disc at the bottom. The cable communicates with a battery and sounding device, such as an electric bell, upon shipboard, and, the sounder being suspended therefrom, normally maintains its vertical position until the vessel approaches a reef or reaches shoal water, when it turns upon its side, establishing electrical communication between the discs, by which an alarm is sounded on board the vessel.

WE have heard for some years past outcries in various quarters about the apathy of the City authorities in lighting some of our main thoroughfares. It seems just now that the piercing rays of the electric arc are not so much required in busy streets as in some of the foul slums in the East end of London. Sir Charles Warren, in a lengthy reply to the resolution recently passed by the Whitechapel District Board of Works, makes the following remark which might have been with advantage more strongly urged :—" I have also to point out that the purlieus about Whitechapel are most imperfectly lighted, and the darkness is an important assistant to crime." It is not too much to say that had the scenes of the fearful atrocities which have recently paralysed the vast population of Whitechapel and Shoreditch been illumined by the electric light, such crimes could not have been committed without discovery

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BARNET seems to be having an unfortunate experience in electric lighting, and it would not be surprising to learn that the new illuminant had been given up in disgust. We trust, however, that the local authorities will not readily give way to despair, for the experiment is one of more than local interest, Barnet being, we believe, the first town in this country in which incandescent lamps in series have been employed. The contractor, not only for his own sake but for that of the company formed to take over his station and effects, would do well to leave no stone unturned to ensure an eventual success.

WE still hear that complaints are rife respecting the practice of the Grosvenor Gallery authorities who, when the demand for current is in excess of the supply, do not hesitate to cut certain circuits off, thus leaving their customers without light. The only wonder is that consumers allow themselves to be so treated.

A GENTLEMAN, who is best known as " the eminent medical electrician," declares in a recently issued pamphlet that alternating currents have no chemical action. As the author of this statement doubtless peruses the REVIEW each week with great relish it seems odd that he did not come across the experiments of MM. Maneuvrier and Chappuis in our issue of July 27th. Perhaps, however, the subject was one which did not particularly interest him, but he should nevertheless make himself acquainted with the current literature of the day before rashly rushing into print with statements which have no foundation in fact.

A NEW weekly paper, entitled *The Illustrated Medical News*, has just been brought out, and the first number appears to us to be a magnificent specimen of technical journalism. We would suggest that special endeavours should be devoted to a determined effort to suppress the ever-increasing trade in bogus magnetic and electric appliances for curative purposes ; and for this, if for nothing else, the latest organ of the healing profession would deserve substantial support.

THE course of instruction for the 1888-89 session of the Finsbury Technical College appears to be very complete, nearly every branch of technical education being well represented. Prof. Silvanus Thompson and his able assistants will doubtless have their hands full during the forthcoming winter, and we trust that the results will be eminently satisfactory to both teachers and students.

WE hear of a new primary battery specially adapted for electric lighting which is likely to come to the fore. Galvanic elements when left to themselves have an unhappy knack of eating off their own heads, but the new combination which has been mentioned to us is said to be practically unaffected by long intervals of inaction. We fear, however, that the report is too good to be true, local action not having yet been completely eliminated in any galvanic couple when at rest, the Schanschieff battery being perhaps the nearest approach to perfection in this direction.

WE have received from the Series Electrical Traction Syndicate a descriptive pamphlet of the system employed, based upon the patents of the late Professor Fleeming Jenkin, Profs. Ayrton and Perry, Messrs. Short, Nesmith, and others. The syndicate is advised by Sir Richard Webster and by Mr. J. Fletcher Moulton that its patents control the absolute monopoly of series-running for traction purposes. A line is being constructed near Gravesend for the purpose of demonstrating all the statements contained in the pamphlet, and this is to be in operation in December. We shall be glad to be kept informed of the syndicate's progress, and we trust it may be more successful than several of the pioneering schemes which have been so unfortunate in practice, for it should now be a comparatively easy matter to avoid the pitfalls into which the early workers in the field so readily fell.

PHONOGRAPH AND GRAPHOPHONE.

WE have before remarked that these instruments do not come strictly within the province of an electrical journal, but their inventors and the circumstances of their invention (and exploitation) are such as to need no apology for our again recurring to the subject.

In a review of the proceedings of the British Association at Bath, the *Times* remarks that their rival claims are more suitable for settlement by law than by science. We doubt if law will have much opportunity of deciding the question, as its delays will afford time for the settling down of what we consider extravagant expectations, and science will have little difficulty in awarding merit where merit is due. Edison's claim to the invention of the phonograph has never been contested.

Edison has made improvements in his own invention. Others have done the same, for which they may be entitled to some credit as improvements, but the transposition of syllables will not make their improvements a new invention.

There is something about the phonograph which inevitably excites enthusiasm, and we are by no means desirous of underrating the merits of the invention or of deprecating the enthusiasm which its results engender. We are desirous only of considering the position which it may assume, the extent of use which may be assigned to it, and of endeavouring, if possible, to prevent the enthusiasm being diverted into unwarranted channels.

Some quicker method of writing than that now adopted has been a dream of inventors and reformers for ages, but the existing system has survived.

Advocates of the phonograph might hold that the transmission of speech itself solves the difficulty. But does it?

It is assumed that the phonographic record may be transmitted and reproduced in preference to being committed to manuscript. To supersede existing methods undoubted advantages must be shown. Do these advantages exist in the phonograph? A letter (particularly a business letter) has to be copied. It has to be filed. It has at some time or other to be referred to. It has to be easily transmitted. It has to be easily read. The phonograph is not at present provided with a copy. It is not suitable for filing. It is not easy of reference. A post office which has had experience in parcels would be equal to its transmission, but for ease there is no comparison with letters. Should the cylinder arrive unbroken it depends for its reproduction on the good order and good management of a delicate instrument.

It follows that it is only unimportant communications that are suitable for transmission by phonograms, and it is not for unimportant uses that new inventions are absorbed by the public.

There remains the use in which the phonograms are not transmitted but are simply used for reproduction at the place where they are produced. Such a use is the one that is contemplated when letters are dictated into the phonograph to be reproduced piecemeal and written out in the absence of the dictator. This is practically what is done by a shorthand writer. To dictate with speed to a shorthand writer needs a special training. It will be found far from easy to talk to a machine.

Would any economy be obtained by the use of the phonograph, or would not the transcriber who had the ability to manage a phonograph command as high a salary as the shorthand clerk?

The original phonograph was an extremely interesting instrument. The present form is much "improved" but it is not "perfected." The instrument will yet undergo many modifications. A return to something like the original form will perhaps enable a copy to be made, will provide a more easy form for docketing and filing as well as more easy transmission. At its best its use will be limited, but it is an invention of importance which in the distant future may find some general use. At present its use can be but special and extremely limited. The Dialectical Society might find one of service in its researches, and there are other cases in which it is conceivable it may be used. We are concerned only in combating the impression which seems to be industriously circulated, that a general and immediate use may be found for it.

THE RECIPROCAL INFLUENCE OF ELECTRICAL CANALISATION.

SOME time ago we (*L'Electricien*) drew attention to the selection of a committee from the Electro-Technical Society of Berlin. The object of this committee was to study the influences exercised by cables traversed by currents of high intensity (*e.g.*, those used for lighting, transmission of motive power, &c.) on conductors traversed by weaker currents (*e.g.*, those used for telephony and telegraphy). The importance of this question will be evident to all. In a future more or less distant we are destined to have a quantity of wires, some used for lighting, and others for telephonic, telegraphic or other applications, which will meet and cross each other at different points. It behoves us, therefore, to take all precautions to avoid as far as possible disturbing influences in the various systems. Thus we are of opinion that the researches made by the Electro-Technical Society, of Berlin, are of the greatest interest; and we will now mention briefly the principal points of their experiments. The various experiments were made upon cables about 500 metres in length, and traversed by different intensities. One of the first experiments undertaken was the following: From the two terminals of the machine (fig. 1) start the two extremities of a concentric cable used for working lamps. This circuit, curved in the form of a U, is 550 metres long, and is traversed by a continuous current of 120 ampères. Inside the inner part of the U is placed a telephonic circuit, consisting simply of a wire covered with gutta-percha. Under these conditions conversation through the telephone was in no way impeded, and no noise was perceptible.

FIG. 1.

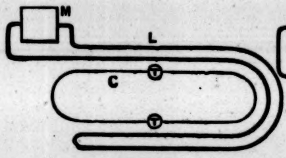
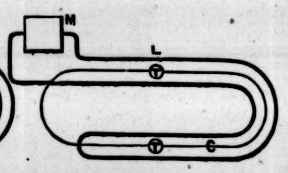


FIG. 2.



M, machine; L, light cable; C, telephonic circuit.

If now, on the contrary, we take a continuous wire, the two cables being about 15 centimetres apart, and the telephonic circuit is placed between them, as indicated in fig. 2, we can hear a slight disagreeable noise, but conversation is not hindered thereby. In the experiment made the intensity of the current traversing the cable was 58 ampères.

FIG. 3.

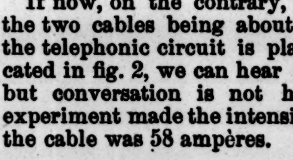
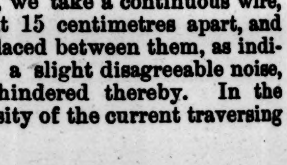


FIG. 4.



M, machine; L, light cable; C, telephonic circuit.

It is no longer the same if we adopt the arrangement represented in figs. 3 and 4. The perception of sounds becomes quite impossible, especially in the fourth case.

The same experiments were made with alternating currents, and the same results obtained. With Messrs. Siemens and Halske's concentric cables conversation through the telephone was not impeded. For the other cases the disturbing effects were more considerable than with continuous currents.

Trials were also made with cables covered with protecting sheathing, and it was found that under these conditions the effects of induction were greatly diminished; but it must be remarked that this method would often be too costly to be put into practice.

In concluding these remarks the committee indicated the conditions to be fulfilled in order to avoid the influences of cables upon one another. They are very simple. As far as possible concentric cables should be employed, or, if ordinary ones are used, the telephonic circuit should be placed at an equal distance from the positive and negative poles of the light cable. These conditions were evident *a priori;* but, when confirmed by fresh and careful experiments, they acquire greater force.

After these researches we can understand the society issuing regulations for electrical canalisation. The conditions imposed are not likely to be dictated by caprice; and it is to be desired that we should endeavour to profit by their proceedings.

DIFFERENT SECTIONAL HEATING OF INCANDESCENT LAMP FILAMENTS.

By LEGH S. POWELL.

THE well-known process of "flashing" has been used in the manufacture of filaments for incandescent lamps for several objects, the principal of which—are the adjustment of filaments to equal resistances, the equalisation of the incandescence throughout the length of the filament, the driving out of the occluded gases which invariably exist in all kinds of ordinarily baked carbon, and for the sake of the bright deposited carbon itself. But may there not be yet another important feature possessed by this beautiful process? Is it not possible that a sensible difference in temperature may exist in different sections of a filament, while it is incandescing, arising through the different respective resistances and masses of the superficial carbon coating and the original filament or core? And assuming for the moment the existence of this difference in temperature in different sections of a filament, is it not also conceivable that, if the relative resistances and thicknesses of the various sections be properly proportioned and handled, considerable improvement in the durability of filaments might be expected to result?

The writer has hitherto seen no reference to this aspect of the subject, and as he is inclined to think it is an important direction for investigation in the further development of the incandescent lamp, he proposes to express his experience of and views upon the subject.

Before entering upon the facts and arguments relating to this matter, it will perhaps be advisable to mention, as a well-known and clearly proved fact, that the specific resistance of carbon obtained by flashing—even if the temperature during deposition be low—is invariably lower than that obtained by the usual methods employed for baking cellulose, which substance constitutes the core for filaments most generally employed. The following particulars of three filaments of the same length, but of different gauges, all of which were flashed at practically the same temperature to an equal resistance, will serve as a general illustration of this fact.

Thin Filament.

Before flashing.	Weight = ·0016 gramme.	Gauge – ·0027 inch
After flashing.	„ = ·0036 „	„ = ·0045 inch·

Medium Filament.

Before flashing.	Weight = ·0028 gramme.	Gauge = ·0037 inch.
After flashing.	„ = ·0044 „	„ = ·0050 inch.

Thick Filament.

efore flashing.	Weight = ·0040 gramme.	Gauge = ·0047 inch.
After flashing.	„ = ·0054 „	„ = ·0055 inch.

Here it will be observed that the final flashed filaments, having the respective gauges of ·0045, ·005, ·0055, will all carry the same current at the same electromotive force.

In order to make the possibility of sectional heating perfectly clear, consider, in the first place, the condition of an original ordinarily baked carbon filament or core, of homogeneous structure and high resistance—such as an Edison bamboo filament—when it is heated by means of an electric current. An equal temperature will be generated throughout all sections of the filament, because of its homogeneous resistance. It is probable, however, that the actual surface is cooler than the interior portions, in consequence of the radiation which is constantly going on. Now suppose the case of a filament of the same length and gauge, but of carbon of lower specific resistance. These conditions will cause this filament to generate heat in an exactly similar manner to the first case, the only difference being that the lower resistance will cause the temperature to be much greater if the same electromotive force is used. Finally, suppose this low resistance filament to be flattened out and to surround the one of high resistance, so as to form, in point of fact, an ordinary flashed filament. The conditions are now entirely changed. The compound filament will be no longer heated in the same homogeneous manner, for now the *surface* will generate the most heat, and consequently should be the hottest portion.

The ready and apparently plausible objection to this assertion will, however, now be advanced, viz., that sectional heating may be all very well in theory, but in practice an interchange of heat must take place very rapidly in so small an object as an incandescing filament, so that, however the heat may be generated, a practically equal temperature will ensue in all the sections. The point is undoubtedly a very difficult one to settle by mere argument, or even by experiment, since there are so many influences at work with each variously constructed filament, and so many different explanations may be given to account for the different phenomena; but the writer is inclined to think that the evidence which will be presented, distinctly favours the view of an appreciable sectional heating, rather than the contention against it.

The policy usually adopted when employing the flashing process, is to deposit but a comparatively small proportion of carbon relatively to the core, so that sectional heating, granting it to be appreciable, will necessarily take place in the way which would appear to be the most desirable, viz., the surface will be the most highly heated portion, while the core will act as a support to the shell; and it certainly is the fact that lamps made on these lines are, to say the least, good ones. Now instead of a tolerably substantial core, let an extremely fine filament be selected, and then let there be deposited on to it, at a low temperature, a very thick and heavy coat: a condition of things will now exist almost the exact reverse of the usual practice, and for this reason: It so happens that the first portions of carbon which are deposited, are of lower resistance than those which follow.* Therefore there exists primarily a very high resistance core of very small and almost insignificant dimensions, and then a coating of low resistance, the conductivity of which decreases as the deposit is thickened. The actual surface, therefore, of this filament, is of high resistance in comparison with its interior, if its diminutive core be omitted. Now on passing a current through such a filament when the lamp is completed, so as to enable it to emit a light equal to 3·5 watts per candle, the sections below the surface will of necessity be more severely heated, owing to the increased current they will carry. This, it is evident, is an undesirable feature from the sectional heating point of view; and there is no doubt that lamps made with such filaments turn out very badly indeed. In point of fact, they have an average life of 200 or 300 hours only, and the bulbs also blacken very rapidly and deeply.

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* Some points connected with this extremely interesting fact were discussed in a paper published in the last volume of the ELECTRICAL REVIEW. See issue for May 18th, 1888.

It is true that other reasons than unequal sectional heating may be given to explain the different results obtained from lamps made with filaments such as those just described, as compared with those made with a heavier core and with a thinner coat. If it be assumed that the temperature of an incandescing filament is practically the same, through communication, in all its sections, the results above mentioned may be attributed to the entirely different qualities of the carbon constituting the respective filaments. In the one case the preponderating description of carbon is derived from baking compact cellulose, whereas in the other it is principally deposited carbon; and the former, it may be contended, is better fitted to stand a high temperature than the latter. The reason why carbon derived from the latter source might be less suited to stand a high temperature—particularly when low temperature flashing has been employed to obtain it—may possibly be that the deposit is in reality a sort of highly carbonised hydrocarbon, such as lampblack is known to be. If this is so, it might naturally be expected to possess different properties to pure carbon. Or the real cause may be owing to the fact that carbon, deposited by low temperature flashing, is more porous than when produced at a high temperature, and it might therefore be in a more volatilisable condition when under the influence of a high temperature and a vacuum. These may be very plausible reasons to account for the different results, and it is not intended here to dispute them; nevertheless they cannot be implicitly depended upon simply on the ground of their suggestiveness, and without any practical evidence to support them; and the writer is not aware of any information on the subject claiming to do this. On the other hand it would at least seem possible that different sectional heating would account for the results above referred to, although if this isolated case were the only evidence in its favour it would not be very forcible. But this is not the only evidence. Very powerful support may be drawn by closely observing the manner in which lamps made with ordinarily flashed filaments disintegrate when the lamps are run for life, some instances of which it is now proposed to furnish.

The filaments of lamps will break under the influence of ordinary running from a variety of causes, some of which appear very obscure. They will sometimes snap when quite new or nearly so. Others, again, not unfrequently break off close to the mount, when in the same recently prepared condition. As a rule, however, there exists in the filament some weak spot which becomes brighter and brighter, until at length rupture occurs, the cause of the unequal brightness being due to an increased volatilisation of the carbon at this point, thus raising the resistance at the locality. But very many lamps made with comparatively stout, sound, original filaments, and coated with a low resistance shell, may be observed to wear out in the following manner: After running a sufficient number of hours a portion, not a spot, of the filament will become a little brighter than the rest.[*] After a further number of hours running this part will become brighter yet, until at length a point is reached when the locality will become duller than the other portions of the filament, instead of brighter, as heretofore. If examined in the cold, the filament, which has all through presented an equally bright and shining surface, will now be found to have a black patch at the locality in question. After still further running the patch will extend, and very likely other portions of the filament will follow the same course in the order just described. The explanation of these phenomena is, no doubt, that the resistance of the smooth shell becames unequal through its unequal volatilisation, and the process continues at a steadily increasing rate, until at length the whole of the deposit at this part has disappeared, leaving exposed, the rougher, and consequently increased, radiating surface of the original filament.[*] Coincident, moreover, with these alterations, a material rise in the resistance of the lamp also occurs.

Another fact which, conjointly with others, strengthens the theory in question, is the following. It has been observed that the filaments of some kinds of lamps are very apt to break at the crown as a result from their use, and it also happens that the frequency of rupture at this part, is greater in filaments which have been heavily flashed. Now the most suggestive method of flashing filaments is to insert one filament at a time into a species of pincers, technically called a "clip," and then to introduce the two into a vessel which has a contracted mouth on its upper surface. Thus, the crown of the filament will face the earth, while the terminals point to the sky. When the facts above stated with regard to the breakage of filaments in lamps occurring most frequently at the crown had been noticed, the cause was not unnaturally attributed to the position occupied by the filaments in the flashing process. By altering the position to a horizontal one, a very marked improvement in the life of the lamps was soon made apparent.[†]

Further experience with horizontally flashed filaments brought to light yet another interesting fact. This is, that after running lamps, whose filaments have been flashed in the vapour of amyl acetate[‡] for a sufficient time, the light emitted from one side or face of the filament is found to be greater than from the other. On examination of the filament in the cold, the side which gives the most light will be found to be still bright and shiney, the other will be black. Subsequent experiment proved that the underneath surface is the one which wears away most rapidly. The explanation of all these facts would appear to be as follows:—When a filament is being flashed a continual stream of gas or vapour as the case may be, keeps rising from beneath it, the circulation being caused by the heat that is evolved. Thus a constant flow of cool vapour necessarily impinges on the crown or lower face, according to the position of the filament, whilst only heated vapours come in contact with the upper surface or parts. The facts relative to the disintegration of these filaments would, therefore, tend to show that carbon is more readily dissociated and deposited from already heated vapours than from cold ones, and this result is precisely what anyone might have foretold. Therefore, it would appear the shell is thicker, in the case of a horizontally flashed filament, on its upper surface than on the lower one. Hence the principal heating of these particular filaments being on the surface, according to the theory of different sectional heating, the thin part of the shell would be just the one to disappear the first. And may not yet another lesson be learnt from the fact that more light is emitted from the shiney side than from the dark one? The still covered side, being of lowest specific resistance, is the most highly heated and therefore gives out more light, whereas if the heat were so readily communicated to all sections of the filament that appreciable differences of temperature were impossible, then the dark side, having the larger radiating surface, should be the one to emit the greater light.

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[*] Small irregularities in the incandescence of a filament are best observed by lowering the temperature to redness.

[*] It may be as well to explain that if an original filament is made into a lamp, and its candle-power measured at, say, 3—5 watts per candle, the quantity of light emitted will be distinctly greater than if it had previously been slightly flashed. The cause of this fact is regarded by some authorities, as being due to a property possessed by a surface of bright deposited carbon, of radiating a larger percentage of light to that of heat at ordinary incandescent temperatures, than that obtained from a surface of black carbon. The writer is inclined, however, to follow the simpler view, that the original filament has a porous, and consequently, a larger radiating surface, whereas the deposited carbon is more compact and vitreous, as it were, and therefore presents a correspondingly smaller actual surface, and thus it follows that the candle-power is lowered. The gauge of either the flashed or unflashed filaments would, of course. be practically identical, and cannot, therefore, be used to determine the actual surface area. In brief, according to the first argument, the temperature of the patch is the same as the rest of the filament, and only appears duller; whereas the second maintains, that the temperature not only appears lower, but actually is lower.

[†] Some curious and interesting phenomena occur on passing a current through a filament placed in a horizontal position. It becomes limp and drops down towards the earth, assuming an arch shape; and the thinner the filament, or the stronger the current, the more apparent does this property become. If, however, the poles of a horseshoe magnet are placed before the filament, so that one pole is below and the other above the loop, the horizontal position will be maintained. The cause of the drooping was at first somewhat thoughtlessly assigned to the attraction caused by the earth's magnetism to the current flowing through the filament, and which attraction was counteracted by a more powerful local magnetic force. But it can easily be proved that this view is incorrect. It is, however, most probably due to a looseness which must occur in the molecules of heated carbon, and to the simple action of gravitation while in this unstable condition; similar, in fact, to what would occur if a fine filament of beeswax were suspended horizontally. Another point in the phenomena may be mentioned, and this is that if the magnet be approached too close to the crown of the filament, the latter is repelled and spreads outwards, becoming bulb-shaped.

[‡] Coal gas and other suitable flashing mediums also show the same effect, but not in so marked a manner.

Yet another similar instance may be given, in which the disappearance of the deposited shell of low resistance may be accounted for owing to its increased temperature. Let a filament be lightly flashed in the vapour of amyl acetate—a substance which produces a deposit of carbon of peculiarly low specific resistance—and let the lamp made with such a filament be run very brightly, say, at 1·5 watts per candle. The filament will be found in the course of an hour or so to have risen very considerably in resistance, and on examination in the cold the bright surface will be seen to have almost completely worn away.

All these instances tend to point to one conclusion, viz., that the surface in these filaments becomes more highly heated than the interior. The most striking case is the one first mentioned, and a little closer examination than has been devoted to it may not be amiss. The phenomenon* of portions of the filament becoming brighter in certain localities, can only be originated by two causes. Either a local increase in resistance must start the disintegration at this part, or else it must be due to unequal local volatilisation of the carbon shell. Of course once the increased brightness has commenced from either cause, the two influences proceed hand in hand in their work of destruction. But with regard to the alteration in the resistance of the filament being the first to start the local brightness, it so happens that the phases of first brightening and then dulling of the locality above described, occur in a far more marked degree in the case of lamps whose filaments have been flashed in partial vacuo. Now, it is a well known fact that the resistance of lamps, whose filaments have been flashed in partial vacuo, is far less liable to alteration than when the hydrocarbon has been at the atmospheric pressure; and it stands to reason, therefore, that the *prime cause* for local brightening must be due to local disappearance of the low resistance carbon shell. But it may be argued thus: It certainly seems clear that local brightening is primarily due to volatilisation, and not a change in resistance; but why should the diappearance be confined to the shell and why should it not equally apply to a general disappearance of carbon from the body of the filament, which part is known to be in a more porous, and therefore possibly more volatilisable condition; this general disappearance producing the necessary rise in resistance to effect the brightening? Several answers may be given, of which the simplest is, that the argument is contrary to fact; for it is the surface which becomes disintegrated; and even if volatilisation did occur more rapidly in the interior, it would be necessary for the carbon to find its way through the dense covering, and it does not seam reasonable to suppose that it would do this. Moreover, it would be difficult to account for the subsequent dulling of the locality, which it has been mentioned sometimes occurs. In addition to these, a still more powerful argument may be brought to bear, viz., that it must be borne in mind that the bright portion is not a point but often a length of half an inch or more of the filament, and to produce a brightening over such an area, would necessitate an appreciable loss of carbon to produce the necessary rise in resistance. But, that no considerable loss of carbon has occurred, is evident from the colour of the bulb which blackens but little during this phase. The same answer, relative to the small loss of carbon, would also settle, without further comment, the suggestion that a local thinning of the filament, from its surface inwards, might be the cause of the brightening. If, on the other hand, the matter is viewed in the light of different sectional heating, it is easily conceivable how a small loss of carbon from the shell certainly would effect a distinct rise in the shell's resistance, when it is remembered that this part is already the hottest, and, at the same time, has but a small bulk in comparison with the rest of the portions of the filament. This aspect is, moreover, further strengthened from the fact that vacuum flashed filaments tend to alter locally in incandescence, on continued running of the lamps, more than those flashed at the ordinary pressure, probably owing to the specific resistance of the former deposit being, much lower than the latter; and this represents a smaller bulk of the shell, and a correspondingly greater difference, therefore, between the temperature of the core and the surface.

In addition to the cases already cited illustrating some of the facts relating to the disintegration of filaments, two additional observations may be mentioned, from which some further light may be obtained relating to this interesting subject.

It has been mentioned, that as a rule, the final destruction of a filament usually occurs through the development of a bright spot. Now if the filament of a lamp containing such a spot be extracted from the bulb, and examined under the microscope at this part, it will present the appearance of an empty honeycomb. In other words, fine round holes will be seen to run right up into the interior of the filament. It would appear, therefore, that when the core of the filament is giving way, the disintegration does not proceed by a peeling off of the surface, as in the cases hitherto considered, but that it takes place through an internal local heating, and the cellular channels apparently serve to convey the volatilised carbon to the surface, whence it finds its way to the bulb to which it finally adheres. And when the circumstances are considered this, surely, is just what would have been expected. The bright superficial coat is invariably absent in a spot of this description, so that sectional heating at this point, in the sense we have hitherto been considering it, cannot occur. Hence the surface, just here, instead of being the hottest portion would now be the coolest, since this part is the one where the pent up heat, generated in the filament, becomes liberated. And the argument may be carried a little further by remarking, that if an appreciably different sectional heating does not occur, why should the brightly coated filaments disintegrate by the peeling off process, instead of by this manner of developing spots, as above described?

The other case is that, with certain descriptions of filaments, a chip of carbon will occasionally fly off from the filament while it is being flashed, thereby rendering it for the moment locally brighter than the rest of the incandescing thread; but the spot gradually heals up as the process is continued, until at length it can no longer be detected. Now, on running a lamp made with such a filament, for life, it not unfrequently happens that the final break will not take place at the presumably weakest part—viz., where the chip has occurred. The deduction to be drawn, it would seem, is that the thickened coat is stronger than an apparently sound portion of the filament, in spite of the section at this part being visibly smaller, and for this reason: at starting, the temperature throughout the length of the filament will be the same, supposing the flashing process to have done its work properly. Now, if deposited carbon had a tendency to volatilise more readily than the ordinary baked description, surely the first indication of disintegration would have become noticeable at this same chipped locality, for the equal temperature would cause an equal loss of carbon from all parts, and this would consequently entail a larger percentage of diminution at a part of small circumference than where the latter is greater. It is true that a large extent of surface will assist rapidity of volatilisation in most ordinary cases, but surely this consideration may be neglected here, where the loss is so minute and spread over many hundred hours. In a case like this the true conclusion to be drawn would appear to be, that if the proportion of shell to core had approached more to that existing at the chipped locality the result, as far as the life of the filament is concerned, would probably have been more satisfactory.

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* It should be stated that lamps exhibiting the phenomena of first brightening and then subsequently dulling again in a certain locality, occur principally in those lamps whose filaments have not been horizontally flashed.

And this remark brings back a theme of considerable importance, but also of considerable difficulty—viz., what proportion the low resistance shell should bear to the interior core. A few general remarks and considerations, however, are all that can be ventured upon with regard to it, for, as remarked at the commencement of the paper, the matter is one requiring further investigation rather than conjecture.

What proportion the low resistance shell should bear to the interior core so as to produce the most satisfactory results must depend entirely upon the quality of the respective carbons employed, and the quality of these may vary in a most perplexing manner, as every lampmaker knows very well. Each different material used as a source of carbon, and each controlling physical condition, such as time, temperature, pressure, chemical agencies, &c., through whose influence the carbon becomes isolated, all combine to produce innumerable variations in the quality, density, specific resistance, &c., of the resulting carbon. To give satisfactory results flashing must be conducted in an intelligent and rational manner, otherwise its evils may far outweigh its many obvious and latent virtues. The very fact that one section of a filament may be more highly heated than another—and the circumstance is tolerably clearly proved to the mind of the writer—must entail a consequent weakness under certain conditions, simply from want of homogeneity. For instance, it is a defect for the flashed surface to be too thin, and therefore to wear off too soon and thus leave the high resistance core to become only red hot at the originally intended electromotive force; and this is not at all an uncommon case, as any casual observer can testify. Or the fatal result attending the very thin core, plus the heavy flashing, may be referred to. But, on the other hand, is it not possible to see, with the mind's eye, a comparatively cool core, supporting a highly heated shell of suitable thickness, and composed of dense and therefore unvolatilisable carbon, and which shall also possess a smooth surface, for the smaller the radiating surface of a given filament the greater must be its stability. Surely this conception is not necessarily unattainable in practice. There must be the proper balance between the two—or possibly more may be desirable in some cases—kinds of carbon. The desideratum is, of course, that both core and shell shall both give way together, and this moreover without having changed much in candle-power. The thicker the shell, the less will a given loss of carbon affect its resistance, and so far so good; but the core has also to be considered, and the thicker the deposit the thinner must be the support, and this must be a move in the reverse direction. Moreover, the thickening of the shell also brings about the unequal sectional resistance of the deposit referred to in the writer's previous paper above alluded to, though this evil might very possibly be obviated. With regard to the resistance the shell should have, there are points which both favour and disfavour the policy of reducing it as low as possible. The lower the resistance of this part in comparison to the core, the greater is the current, and therefore temperature, on the surface, and this is unquestionably a desirable result; but a very low resistance shell entails a thinner and consequently a less stable one, and surface temperature should not be obtained at too great an expense in this direction. Then with reference to the supporting core, it should without doubt be of as high resistance as possible; but it would probably be a very false step to increase the resistance by rendering it porous, for although it does not generate the most intense heat, still there is no doubt that it must become exceedingly hot, and therefore more liable to give way. In the words of an Irishman a porous and *loose support* cannot be an ideal one.

It is always possible that after considerable experiment in this direction, filaments might be constructed of excellent quality, and yet still be impracticable to manufacture from expense of material or labour, and in consequence, have to give way to less costly and expeditious methods. But when experiments and observations, of the nature of those here described, show the strong probability of the existence of these different temperatures in the sections of certain filaments, the subject is certainly one worthy of attention, for in spite of possible failure, the outlook for improvement appears on the face of it to be promising.

It should be added, that throughout this paper, the writer has assumed that carbon volatilises. Perhaps this assumption is rather a rash one, seeing that carbon does not even fuse in the electric arc, where the temperature is infinitely higher than in an incandescing glow lamp. But there is no doubt that visible alterations take place, in the aspect of filaments, and of the bulbs, in the course of the lives of lamps, and it is but natural to suppose that carbon becomes transferred from the one to the other through the agency of extremely slow but ordinary volatilisation, in the same way as arsenic will do. Moreover, the existence of a vacuum would assist matters.

EGYPT AND THE TELEPHONE.

WE learn from the *Bosphore Egyptien* that a telephone line has been installed to Cairo and Alexandria, and great exultation is shown in Belgium that a Belgian system, that of Van Rysselberghe, has been selected, one of the Brussels journals remarking:—"Recently we were the first to announce the establishment in Portugal of a telephone line on the Van Rysselberghe system. We learn that a new application of the invention of our learned compatriot has just been realised with complete success between Cairo and Alexandria. We may add for the honour of our national industry, that in Egypt as in Portugal, all the material employed is of Belgian manufacture, and comes from the workshops of Mourlon, of Brussels. The conductor wires of the line from Cairo to Alexandria being of Montefiore phosphor bronze, it has been found possible to utilise for communication between the lines of these two places the iron telegraph wires, and that without deranging anything in their first destination. This is the system which has been applied on all the Belgian State lines, and which gives results so much better than some others which are spoken of as being in a higher state of perfection. From this point of view the use of the Dejongh transmitter cannot be too much recommended for adoption by our telegraph administration for its Ostend line, and for communication between Paris and Brussels. It must not, however, be lost sight of, that above a certain distance words cannot easily be transmitted by an iron wire, and that has decided the Belgian Government to replace proportionally, to wear and tear, iron wire by that of phosphor bronze. When this substitution is accomplished, our telegraphic lines will have, thanks to the Van Rysselberghe system, a double use, telegraphic and telephonic, so that the working, instead of being carried on at a loss, will result in important profits."

Telegraph Enterprise.—It is rumoured that so soon as the financial arrangements have been completed the Antilles Cable Company intends to extend its system from Cuba to some point on the Mexican coast, and touching, if landing rights can be there obtained, at Vera Cruz.

A SYNTHETIC STUDY OF DYNAMO MACHINES.

(*Continued from page* 302.)

II.—LAWS OF INDUCED E.M.F.—(*Continued.*)

THE value of the induced E.M.F. for every position of the frame during its revolution can be represented by a diagram, fig. 30. Here the line, A, B, represents degrees, and is supposed to be divided into 360 parts. Distances along the line from A represent, therefore, the angle through which the frame has turned, and ordinates the value of the induced E.M.F. for each position. This is proportional to the sine of the angle, and rises from 0 to a maximum at 90°, falling to 0 again at 180°. The induction now takes place in the opposite direction, and starting from 0 at 180°, attains its maximum value at 270°, falling to 0 again at 360°. The curve in fig. 30 is called a curve of sines, and when the induction is exactly represented by such a curve, it is said to follow a simple harmonic law.

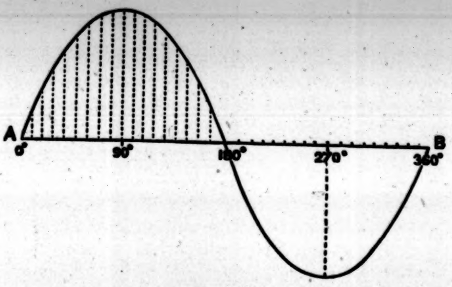

FIG. 30.

11. *The E.M.F. induced by transverse motion in fields of varying intensity and sign.*—Suppose by some means we produce a field of intermittent character, as shown in fig. 31. Here the lines of force are grouped in bundles, separated from each other by spaces, in which there is an absence of lines. The movement from position A to C has the effect of subtracting from the rectangular frame all the lines in group A, and afterwards adding to it all the lines in group C. The frame is displaced to the right in positions B and C to make the diagram clearer. If there is in groups A and C the same number of lines, the frame has, in passing from one position to the other, performed a cycle, being in the same condition at C as it was at A. As it moves from A an E.M.F. will be induced which would send a current round the frame in the direction *a*, *b*, *c*, *d*, that is to say, the face by which the lines of force enter is made by the current a S pole. The E.M.F. would remain fairly constant until the frame arrives at B, since the rate of subtracting lines of force would be uniform for a uniform speed. At B the direction of induction changes, since lines of force are now being added to the frame. From B to C the E.M.F. would again remain constant until at C there is another change, and the cycle is repeated. The only part of the frame cutting lines of force from A to B is *a*, *b*, and the current, therefore, flows from *a* to *b*; but from B to C the part inductively active is *c*, *d*, and the current flows from *d* to *c*. Observe that the current always flows in the same direction in the inductively active part, because it is always cutting the same kind of lines of force in the same direction. But the current flows in alternating directions round the frame, because one side and the other are alternately active.

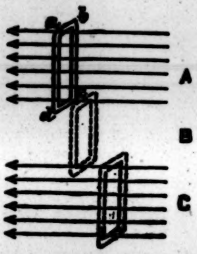

FIG. 31.

Calling N the number of lines of force in each group, it is evident that in a cycle the frame cuts 2 N lines, and supposing the rate of traverse to be such that there are n cycles per second, the average E.M.F. in volts is evidently $= \frac{2\,\mathrm{N}\,n}{10^8}$.

Suppose that the field instead of varying in intensity changes sign or takes the form shown in fig. 32. Here we have a group of lines in one direction and a group in the opposite direction alternately. Let the number of lines in each group be N, as formerly, and let the frame perform a cycle which in this case means a movement from A to C. In moving from A lines of force will be subtracted from the right face of the frame, but at the same time added to the left face, which will serve to increase the induction. This will continue until it arrives at B, where the direction of the induction will be reversed. We shall then subtract from the left face and add to the right face until we arrive at C, when the direction of induction is again reversed and the cycle repeated. In moving from A to B, the side of the frame, *a*, *b*, cuts all the lines of force in group A, and a current is induced in the direction *a*, *b*, *c*, *d*. At the same time the side, *c*, *d*, cuts all the lines of force in group B, and since in this group the lines are in the opposite direction, the induction assists that in *a*, *b*. The lines cut from A to B are therefore 2 N, or in a cycle 4 N, and if there are n cycles per second the average E.M.F. in volts is obviously $= \frac{4\,\mathrm{N}\,n}{10^8}$

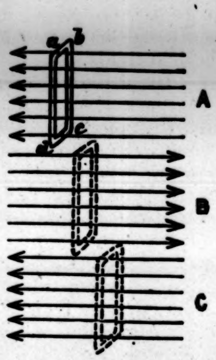

FIG. 32.

We might endeavour to represent the E.M.Fs. graphically in these cases. Looking at fig. 31 it is evident that the reversal is very abrupt, there is no gradation in the changes as we found when the frame was rotated in a uniform field. When the period of induction in one direction is completed, the period of induction in the other at once commences, and the E.M.F. rising almost instantaneously to its full value, remains, as we have seen, constant throughout the period. We have therefore to represent the E.M.F. at any instant during a cycle the diagram shown in fig. 33, ordinates repre-

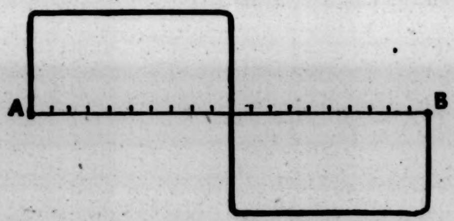

FIG 33.

senting E.M.F. and distances on A B subdivisions of the cycle. For the induction in the field of fig. 32 the diagram must be of a similar character. The change here is also abrupt, but since the average E.M.F. is doubled, the ordinates have twice their former height.

Lord Rayleigh very properly pointed out in his letter of last week that a magnetic field (as in fig. 25) of varying intensity with the lines running straight across all the way is a mechanical impossibility, and the same might be said for most of the fields here given. As a matter of fact, however, the lines in the field to which Lord Rayleigh refers will run parallel to each other for a very short distance, and it is only in the immediate vicinity of the frame that we wish them to be understood as straight. All these fields are purely imaginary, and we have not considered how they are produced, but having familiarised ourselves with the induction in them, we shall consider later in what respect the fields in actual machines differ from them. We shall also refer later to the curves of actual machines which differ for several reasons from the diagrams here sketched for hypothetical fields.

12. *Self-Induction.*—In all our considerations we have spoken of the induced E.M.F., having mentioned the current only for the purpose of indicating the direction of induction. It is now necessary that we should consider the current which flows as a result of the E.M.F. induced. Hitherto we have assumed that the E.M.F. at any instant can be deduced from the rate of cutting the lines of force in the magnetic field, but it is only under special circumstances that this is true. If the frames we have been considering were slit at some point to prevent a current from flowing in them the E.M.F. could be ascertained exactly from a knowledge of the rate of cutting lines, but when a current flows there are induced lines due to the current flowing in the frame itself, and the current must be considered as resulting from the cutting of these lines combined with the lines of the field.

Reverting to an early experiment, fig. 3,* we found that when the coil, C, was moved away from the N pole of a magnet an induced current flowed momentarily in the direction of the arrow. The induced current was due to the sudden withdrawal from C of N lines of force, which proceeding from S, N, entered the left side of the coil. The current flows in C to make its left face a S pole, and creates lines of force in the same direction as those from the magnet, which lines by the current are increased, while by the motion those of the field are being reduced. An E.M.F. is therefore produced opposing that which would be produced by the field alone. If the coil is moved towards the magnet the lines due to the current are thrust in at the right side, which again produces an opposite effect to the motion, which thrusts the field lines in at the left side. This induction of lines through the coil by a current flowing in the coil is termed *self-induction.*

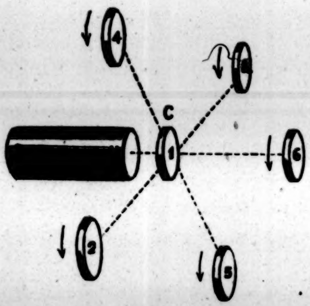

FIG. 3.

E.M.F. can be produced only by change in the number of lines of force passing through a coil, and as long as the number of lines due to self-induction remains unchanged no back E.M.F. will be produced. But the self-induced lines at any moment are strictly proportional to the current flowing, and any change in the current immediately induces a back E.M.F. which acts in all cases to oppose the change of which it is the effect.

(*To be continued.*)

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* Fig. 3 is here repeated for the convenience of the reader.

ELECTROLYSIS OF TIN SALTS.

By ALEXANDER WATT.

(*Concluded from page* 333.)

IN the last paper it was stated that there was some difficulty in obtaining tin crystals from sulphate of tin solutions, from which photographic prints could be produced direct, owing to the tendency of the crystals to grow vertically upon a flat surface as well as horizontally—a peculiarity which also occurs with some lead solutions, but notably with the nitrate of that metal. Wishing to reproduce crystals deposited from the sulphate of tin solution by the photographic printing process, if possible, I made some further attempts, and after a good many failures, succeeded in obtaining a few fairly good results. To form an electrolyte, half an ounce of sulphuric acid was added to eight ounces of water; this solution was poured into a flat dish, in which was laid a glass plate, 7 × 5 inches, the liquid being allowed to cover the plate to the depth of about one-eighth of an inch. A tin anode, connected to the positive pole of the two Daniells in series, was now laid on the plate, and the negative wire of the battery placed at a short distance from it. With this arrangement, a spongy deposit of tin appeared at the cathode in about 15 minutes; the anode was next shifted to about four inches from the cathode, the spongy metal having been previously removed from the end of the wire, when in a few minutes after the spongy deposit again appeared on the cathode. The anode was then brought within $\frac{1}{16}$th of an inch of the negative wire, and steadily held in that position, when a series of well-defined crystals immediately darted forward from the spongy mass, and were seen to move, with a wave-like motion, in the direction of the anode, when it was gradually drawn away from the cathode to prevent the deposited metal from coming in contact with the positive electrode. In the course of a minute or so, the delicate crystals, which were of a dark grey colour, rose to the surface of the liquid, being buoyed up by bubbles of hydrogen which had collected beneath them, and thus the progress of

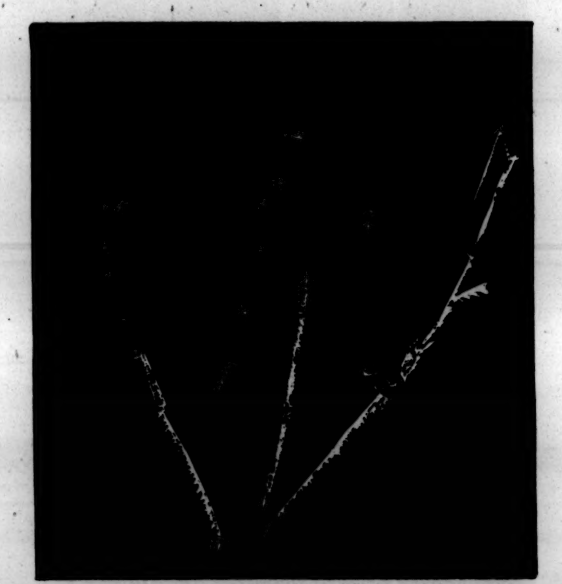

FIG. 7.

the experiment was checked. The crystals were now wiped off the end of the wire, which was then replaced, and, as before, brought as near as possible to the anode, without touching, when in a moment crystals again formed, but still more rapidly, and in about half a minute a deposit nearly an inch in length and three-fourths in width appeared, which, as before, rose to the surface of the liquid, and again temporarily suspended the operation. Having thus noticed that definite crystals could be induced to spring out from the base of the spongy deposit, I next determined to strengthen the bath by adding a small quantity of a strong solution of sulphate of tin, in the hope of getting a group of crystals, *beyond the spongy boundary*, without manipulating the anode in the manner described. To accomplish this, the electrodes were placed about five inches apart, and the bath left undisturbed for about two hours, by which time the following result was obtained: surrounding the negative wire there was a spongy deposit of about one and a half inch in diameter, from the extreme edge of which crystals, more or less well defined, had grown, extending in all directions, but of course more especially in the direction of the anode. The result of this experiment is shown in fig. 7, the black oval space at the base of the crystals indicating where the spongy mass was formed, and which it was found necessary to remove in order that the photographic paper might lie as close as possible to the crystals in the after process of printing from the plate.

FIG. 8.

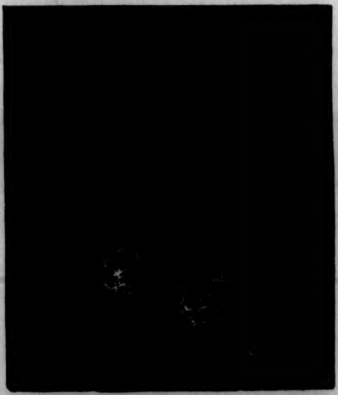

FIG. 9.

In figs. 8 and 9 are shown examples of crystals obtained from the sulphate solution previously described, the first of which (fig. 8) was obtained with a wire cathode $\frac{1}{16}$th of an inch in section, and the second (fig. 9) with stouter wire. In the three illustrations just referred to it will be seen how greatly the crystals obtained from the sulphate of tin solutions differ from those of the chloride, represented in the first paper of this series.

20. *Chlorides of Tin and Ammonium.*—A solution of protochloride of tin was first made, containing $7\frac{1}{2}$ grains of the salt to each ounce of water. Sixty grains of powdered salammoniac were then dissolved in 10 ounces of the solution, and the liquor slightly acidified with hydrochloric acid. The bath thus formed was worked with a single Daniell cell, when a very prompt and white deposit of tin was obtained upon a brass plate, the anode surface being about one-sixth that of the negative plate. The film retained its whiteness, however, only for a few minutes, when the deposit gradually darkened and assumed the spongy or non-reguline form. The solution was next diluted with nearly an equal bulk of water, when deposition proceeded somewhat more favourably, and the metal retained its reguline character for a longer period than before; after a time, however, the surface of the plate became coated with a pulverulent deposit, which was readily wiped off with the finger. The plate was then scoured with moist silver-sand and returned to the bath, and was kept in constant motion for about 10 minutes, the anode surface having been somewhat increased, when it was found that the deposit was quite free from sponginess and perfectly reguline. The plate was again allowed to rest undisturbed in the bath for a few moments, when the coating soon assumed the grey and non-adherent variety of the metal. Another solution of the chlorides of tin and ammonium was next tried, in a somewhat more concentrated condition, when the metal at once deposited in the form of minute crystals, which covered the plate in a few seconds, while a group of crystals quickly started out from the edges of the plate, and in about 15 minutes after nearly touched the anode, when they gradually drooped and sunk to the bottom of the vessel, where they accumulated with great rapidity, forming a glistening cluster of great brilliancy. This experiment verified what had been before noticed—namely, that the weaker solutions yield smaller crystals of the metal, forming a spongy mass, while they assume larger dimensions and more definite forms in liquids richer in metal. In fig. 10 an example is shown of crystals obtained in the stronger bath of the composition given, in which it will be observed that a new crop of crystals appears to grow out of the truncated apex of the upper group of crystals—those which were nearest to the anode. I have found that the growth of these crystals can be considerably modified by altering the position of the electrodes; for

FIG. 10.

example, if the crystals are allowed to approach the anode until they nearly touch this electrode, they invariably assume a flattened appearance, or as if they had been cut off at these points ; if now the anode be drawn away from the crystals to the extent of an inch or two, in a few moments after we shall observe that a fresh growth will spring out from the truncated crystals, and, in some cases, these will be of an entirely different character to those of the original group.

21. *Tartrate of Tin.*—A bath was prepared by digesting moist carbonate of tin in a strong and hot solution of tartaric acid, the resulting liquid being afterwards filtered and moderately diluted. With the current from a single Daniell cell a bright and very white deposit of tin was at first obtained, but this gradually turned dull, and after a few minutes' immersion of the plate the deposit was found to be powdery and non-adherent. The metal, however, was readily obtained in the reguline form, and perfectly white, by keeping the cathode in motion while deposition was progressing.

22. *Oxalate of Tin.*—A solution of this salt was formed by digesting moist hydrated protoxide of tin in a hot and strong solution of oxalic acid, the liquid being afterwards filtered and moderately diluted. With the current from a single cell a brass cathode received a prompt deposit of tin from this bath, the immersed anode surface being somewhat larger than that of the negative electrode. The deposited metal was of a good white colour, though dull, and perfectly reguline.

23. *Peroxalate of Tin.*—To prepare a solution of this salt, a quantity of moist hydrated peroxide of tin was digested in a hot solution of oxalic acid, and the clear liquid when cold was sparingly diluted and electrolysed with a single Daniell, when a brass plate received a coating of tin, but not of a very good colour, a few seconds after immersion. The deposit was somewhat irregular and patchy, but otherwise perfectly reguline.

24. *Borate of Tin.*—Although this salt usually occurs in the form of an insoluble powder, a small percentage of the hydrated protoxide, or of the carbonate of tin, becomes dissolved in a nearly boiling solution of boracic acid, and the resulting liquid, electrolysed with one or two Daniells, yields a deposit of metallic tin upon a brass cathode. Beyond this fact, there is nothing of interest in the electrolysis of this solution.

25. *Nitrate of Tin.*—A solution of this salt was formed by digesting recently precipitated and moist hydrated protoxide of tin in dilute nitric acid, the liquid being afterwards filtered and diluted with water. When electrolysed with the current from a single cell, a film of tin was received upon a brass plate soon after immersion, but shortly after a yellow deposit formed on the plate, while the anode became somewhat thickly coated with a white insoluble substance—probably a basic salt. The solution being nearly neutral, a few drops of nitric acid were added, and a fresh plate immersed, when a bright and very white film of tin was obtained on a brass plate ; this, however, soon became dull, and a yellowish film again formed on the face of the cathode and reguline metal ceased to deposit. It was noticed that in this solution the anode entirely lost the crystalline appearance which it retained in almost all the other solutions tried, while a somewhat firmly adherent and perfectly white salt coated the anode, giving it the appearance of enamel.

26. *Pernitrate of Tin.*—Recently precipitated peroxide of tin was digested in dilute nitric acid, and it was observed that the oxide was more freely soluble in a weak than in a strong solution of the acid. A nearly neutral solution of the persalt having been obtained, the current from a single Daniell was passed through it, when it at once became apparent that the liquid became decomposed, a brownish-yellow deposit having formed upon the anode, which rapidly dissolved in the liquid, imparting to it a yellow colour. There was no metallic deposit upon the cathode, but a white gelatinous mass formed upon its surface ; the bath was now allowed to rest undisturbed for several hours, at the end of which time it was found to have assumed a very peculiar appearance : surrounding the anode, and projecting beyond it to the extent of several inches, a profuse light brown mass had formed, while the whole of the liquid beyond this boundary had been converted into a white gelatinous mass—the contrast between the substances formed at the anode and the cathode respectively being exceedingly marked. On examining the cathode at the conclusion of the experiment, it was found to be very slightly coated with tin of a dark grey colour, while beneath the light brown deposit formed at the anode there was found an opaque white stratum, which came away from the plate in hard flakes when scraped with the finger nail. On removing this layer, the underlying surface of the anode presented a brilliantly white appearance, but quite free from the characteristic crystalline appearance which tin invariably assumes when electrolysed in either acid or alkaline liquids. It thus appears that no less than three different forms of tin are produced during the electrolysis of the pernitrate of that metal, two of which, one of a light brown colour and the other white, are formed at the anode, while the remainder of the liquid becomes nearly solid from the formation of a white gelatinous matter, probably a basic salt. It seems difficult to account for the presence of two different deposits at the surface of the anode, but more especially for the light brown colour of the deposit overlapping the white layer, which latter was closely attached to the surface of that electrode.

27. *Ferrocyanide of Tin in Caustic Potassa.*—A bath was prepared from this combination as follows :—A solution of ferrocyanide of potassium was added to another of protochloride of tin until the whole of the metal was precipitated as ferrocyanide, which was then filtered and washed in the usual way. The resulting mass was next dissolved in a tolerably strong solution of caustic potash, and the liquid afterwards considerably diluted with water. The bath thus formed was tried with the current from a single Daniell cell, when a prompt and pearly-white film of tin was at once received upon a brass plate, the electrode surfaces being about equal. It should be mentioned that before the bath was put into good working order, increasing proportions of water required to be added, until the solution had become exceedingly weak, in which condition it yielded very satisfactory results, while the anode kept perfectly clean. A clean plate of iron was next placed in the solution, but it was found to become coated so slowly that a second cell was connected, when the iron surface quickly became coated with tin. It is probable that a tinning solution prepared from the above combination might be found useful, and if so it would also have the advantage of being cheaper than some of the baths which have been employed for electro-tinning.

28. *Ferrocyanide of Tin in Cyanide of Potassium.*—A solution was prepared as above, substituting a strong solution of cyanide of potassium for the caustic potash, but the solvent action of the cyanide is so far inferior to that of potash, that its employment cannot in any degree be recommended. Although tin can be deposited from the double cyanide solution of a pearly whiteness, a bath of this composition would be of no service for practical purposes.

29. *Cyanide of Tin.*—The hydrated oxides and carbonate of tin are so feebly attacked by even very strong solutions of cyanide of potassium, that we may say that tin solution prepared with the cyanide would be absolutely useless for the deposition of the metal.

30. *Peracetate of Tin.*—Having noticed the peculiar behaviour of the pernitrate of tin under electrolysis, it was determined to see what results would be obtained with the peracetate, for which purpose a small quantity of solution was prepared by digesting moist hydrated peroxide in a strong solution of acetic acid, moderately heated. The resulting liquid, after filtration, was tried with the current from two Daniells, when a white film of tin slowly formed upon a brass cathode, the deposit eventually assuming a dark grey colour. There was a rather brisk evolution of hydrogen, and no non-metallic product formed at either electrode.

In the foregoing experiments, while ascertaining the behaviour of various tin salts under electrolytic action, I was also desirous, if possible, to discover some preparation of tin which would be less open to objection than the salts ordinarily applied for depositing the metal, and if I have not succeeded in securing a formula for an absolutely reliable tinning bath, it is possible that some of the results given may be worth further attention at the hands of those who are specially interested in the electro-deposition of tin for commercial purposes.

THE ELECTRIC LIGHT v. GAS.

WE notice in the *Annales Télégraphiques* some particulars given by Messrs. Brun & Co., owners of a weaving, braiding, and silk manufactory at Saint-Chamond (Loire), as to the comparative cost, obtained from 18 months to 2 years' work, between gas and the electric light at their establishment.

We are told that the original lighting of the factory consisted of 540 gas jets, consuming 20,000 francs worth of gas annually. These jets were removed and replaced by 600 incandescence lamps.

One-half of the lamps are Edison, the remainder Swan; their average life is about 1,200 hours.

The current is supplied by an Edison dynamo of 450 ampères and 100 volts. It has worked for 18 months on an average of 15 hours per day.

A portion of the factory works day and night, thus some of the lights are in use for 3,600 hours per year, while others have only 600 hours' work.

Cost of engine of 90 H.P., foundations, masonry, pipes, belts, pulleys, &c.	32,000 francs.
Dynamo, conductors, lamps, &c.	23,000 „
	55,000 francs.

The yearly cost of lighting comes to :—

10 per cent. sinking fund on 55,000 frs. ...	5,500 francs.
5 per cent. interest	2,750 „
Increase in coal consumption	1,200 „
„ „ oil, &c.	250 „
Renewal, 600 lamps	2,700 „
	12,400 francs.

An economy of 7,600 francs per annum is consequently effected by the use of the electric light. Labour has not been included in the yearly expenses since the number of mechanics employed previous to the installation of the electric light, has not been increased.

LIGHTNING CONDUCTORS.

(*Continued from page* 354.)

Lord **RAYLEIGH** said: I have recovered my equanimity a little during the time that Prof. Lodge and Mr. Abercromby have been speaking, because during Mr. Preece's address one felt that "mathematician" was becoming almost a term of rebuke. I suppose that it is as a representative of the mathematicians that I have been called upon to take a part in the discussion. No doubt there is a good deal of dislike on the part of the public to mathematics, much of which I expect is the result of sufferings undergone in youth over Euclid and similar instruments of torture. I remember at Cambridge it was the custom often, after the "little go" was passed, to celebrate the occasion by the burning of Euclid, and I can to a certain extent understand the feeling, for I entertained very much the same sentiments towards Latin and Greek. But, seriously, whatever may be the failings of individual mathematicians—and no one will deny that some mathematicians are rather unpractical in their work—it is to mathematics one must go if one wants to know what will be the operation of known causes under new and unfamiliar circumstances. There is no other means of going to work. Just as an engineer will have to apply mathematical methods, even if he does not go beyond the rule of three, to the calculation of a bridge, so we require mathematics, no doubt often of a more complicated and difficult character, to foresee what the influence of electrical and other forces will be. Mr. Preece spoke of the mistakes that mathematicians have made, and no one can deny that they have made plenty. But I do not know why we should recognise Dr. Lardner specially as a great mathematician. He it was, I think, who made the prophecy about the impossibility of crossing the Atlantic. The only book of Dr. Lardner's that I have ever read was rather an engineering book than a mathematical one—it was on the steam engine. My thoughts go back to a well-known book, "The Life of Stephenson," by Smiles, in which a great authority of the day is said to have offered to eat a stewed engine wheel for breakfast if the locomotive ever attained a speed of twelve miles an hour. That, I believe, was a great engineer, and not a mathematician.

But enough of recriminations. We all know that engineers have their important work to do, and most of us would admit, I think, that there is work also for mathematicians to do which engineers are not so competent to perform. As to the more particular question, I have had no experience whatever of lightning conductors, and can only speak from a general knowledge of electricity, no doubt to be applied in this case to very peculiar circumstances, so that everything that stands upon a merely *à priori* foundation should be put forward with very great diffidence. I must say, however, that Prof. Lodge's experiments do seem to me to be likely to have most important practical applications to lightning conductors. I cannot see how experiments dealing with the thing of all others most like lightning that we can produce in the laboratory can fail to have such an application. Prof. Lodge asked one question, and I think mentioned my name in connection with it, as to how it could happen that an oscillatory electric current—an alternating current beginning at a finite magnitude and gradually dying away—could produce magnetising effects such as we well know it does in some cases; and he instanced the very opposite behaviour of slowly dying away alternating currents in the experiments of Prof. Ewing and of others, in which such an arrangement is precisely the one adopted in order to get rid of even the last traces of magnetism. The question is a difficult one certainly, and I had intended, in fact, to make some experiments upon it myself; but I am inclined to think that the explanation may, perhaps, be sought in a point very much akin to the one with which we are now dealing, namely, that the steel needle which is to be magnetised, say, by the discharge of a Leyden jar flowing through a spiral enveloping the needle, is itself a conductor of finite dimensions, and I have no doubt that in some cases of alternations of electric current through the spiral, there are induced in the steel needle itself, which ultimately becomes a magnet, circumferential currents, which currents would at first, at any rate, protect the interior of the iron from the direct magnetising action of the enveloping helix. We must think not only of the direct action of the oscillating current in the helix upon the various parts of the steel needle, but also of the action of the currents developed in the needle itself. I believe it has been well ascertained, that at different depths in such a steel needle you will very often find different degrees and even different directions of magnetisation. It seems to me possible that if this was thoroughly followed out we should be better able to understand what is certainly a fact—that a current whose whole value, in some cases measured algebraically, is zero, and is certainly alternating and gradually dying away, does, nevertheless, produce and leave behind it the effect of strong magnetisation.

There is only one other point which it has occurred to me to mention in connection with the development of atmospheric electricity, and I should like very much to hear some meteorologist express his views upon it. Mr. Preece spoke of the great energy which we have to draw upon in the condensation of large quantities of vapour into water. There is no doubt that there is no difficulty as to the total amount of energy at work in the atmosphere : the difficulty is to see how any of that energy takes the electrical form. I was reading the other day a pamphlet by Prof. Somcke, a man who has done very good work in other departments of science, in which he develops the theory that atmospheric electricity is due to friction between water and ice. There is no doubt that many clouds (cirrus clouds, I believe, nearly always) are ice clouds and not water clouds. They consist of spiculæ of ice, and the fact is proved by the peculiar optical effects of ice crystals in producing various kinds of halos and coronæ. There is no difficulty, optically, in distinguishing an ice cloud from a water cloud. Dr. Somcke's view is that at a certain level in the atmosphere ice clouds and water clouds may meet, and that under those circumstances this friction may occur ; and his view is that the atmospheric electricity is the result of such friction. He quotes experiments by Faraday on the action of friction between ice and water, in which it has that result.

But that is perhaps not the main question before us. I would rather hear some more experiences of practical men who have been at work on lightning conductors, especially in circumstances of the kind that Prof. Lodge asked for of actual failures of lightning conductors. It seems to me that it is only by actual experience of lightning conductors that the question will ever be finally settled. The laboratory experiments must be most important as suggestions, but no one would wish finally to adopt any system or to change any system of lightning protection without having some experience upon a large scale.

Sir **WILLIAM THOMSON** said : I must say, sir, that I felt like a highly-charged Leyden phial until Lord Rayleigh beneficently acted as a lightning conductor, and almost wholly discharged me in respect to the questions raised between mathematicians and engineers by Mr. Preece. I am sure we might have felt very angry, but I am also sure that we did not. I do not think Mr. Preece would ever make anyone very angry, and certainly I do not think he intends to do so. I would just like to say this emphatically, that it was not a mathematician but a very great engineer who predicted that the Atlantic cable could not be laid. A good many sailors predicted that the Atlantic cable could never be laid. Some sailors thought it could be laid, and took part in it and did lay it. No mathematician ever predicted that it could not be laid ; some, on the contrary, rather calculated beforehand that it might be laid. Their calculations, of course, were not of any value in the face of the practical experience which led to the laying, but I well remember how one great engineer—the greatest engineering name of the day almost—announced that it was impossible that gutta percha could stand the pressure, and altogether that the whole thing was a chimera, and that the Atlantic cable could not be laid. While that engineer was on board ship in the roadstead of Cherbourg in 1858, on the occasion of the Queen's visit to France, the news came across that the Atlantic cable had actually been laid, and it was like a discharge of lightning to him. It is only right that the mathematicians should be exonerated from the charge that they had predicted so many failures ; because, as a matter of fact, they did not predict any one failure in any one of the instances adduced; but I do not think that is a matter of very great importance now. I was, however, interested in what Mr. Preece said, and also very much interested in Lord Rayleigh's remarks calling attention to the genuine alliance between mathematicians and engineers.

In respect to the very essential difference between Prof. Lodge and Mr. Preece I may say that I think Prof. Lodge is in the American stage with reference to judging of the functions of inertia, and Mr. Preece rather in the English stage, as described on Friday evening by Prof. Ayrton. We call it retardation in England ; the Americans call it going ahead. The two functions of inertia are to prevent anything from getting into motion, and when in motion to keep it going ; and both those functions are thoroughly performed in this electro-magnetic influence—also called self-induction. I cannot but think that if Prof. Lodge continues his work he will find the explanation of the very great discovery he has made, that iron wire affords a better discharge, or an easier path in the circumstances that he has minutely defined, than does copper. I hope he will pursue the investigation, keeping the circumstances in all respects as similar as possible, comparing, for instance, a thin iron wire with a thin lead or brass wire of precisely the same ohmic conductivity. He has done a good deal already, but a good deal more may be done by taking a proper account of inertia and of the other point Prof. Lodge very importantly accentuated that the energy must be got quit of somehow and somewhere. It must be got rid of either in the conductor or elsewhere ; if got rid of in the conductor then there must not be energy enough to melt the conductor, and it may be a positive advantage to have a quasi-conductor to keep it oscillating for a time.

It was an interesting fact about the number of horses in a row, where the first and last in the row were killed and the others not injured by a lightning discharge which seemed to pass through them all. A very common lecture room experiment is to give a Leyden jar shock to a hundred or two hundred students sitting on the benches, making them all join hands and completing the circuit. I have no doubt that those next to the ends of the line experienced the shock much more potently than those in the middle of the line. There is one very marked influence here, and that is want of perfect insulation ; but there is another influence, also potent—self-induction. The influence of self-induction may be tried, and it would be a very interesting experiment. I think we might try this same thing with as nearly as possible similar insulation with the same number of people spread out in a large circle. Then self-induction will be much more influential in causing the discharge not to keep through the line of the comparatively good conducting bodies, but to spread through the floor or ground under them. Take the case of persons ranged in a row and receiving an electric shock. If the row is zig-zag then self-induction will not have the same tendency to cause the lightning discharge to leave the line of the best conductors, as it will have if the conducting bodies are placed in a wide circle. I believe the imperfect insulation of standing on the floor would be much more potent, and should expect that those who were in the middle of the row would experience less of the shock in the case of standing in a wide circle than in the case of standing on similarly good conducting material in zig-zag rows. At all events that is an experiment worth repeating.

Mr. Preece spoke of the impossibility of conceiving of the enormous augmentation of potential in these actions. Take the name "inertia" which Mr. Preece used ; I fully agree in the judiciousness of that term. Apply the word "inertia" to the steam hammer and the hydraulic ram. You begin with a comparatively gentle force, its weight bringing down the steam hammer, till it produces an exceedingly intense action at the blow. Take the hydraulic ram, the well-known analogue for getting up a high potential for self-induction in an electric circuit, and then I think you will see that Prof. Lodge's explanation of the phenomena he has brought before us is altogether valid.

Exceedingly interesting questions have been put, and remarks made, about oscillatory discharge in ordinary lightning, and also with reference to the duration of the charge and to multiple flashes. I think these must be distinguished more or less from one another. There may be a slow discharge. Mr. Abercromby has referred to a ball of lightning running about down the street and so on ; and there is a curious description in Arago which Prof. Tait credited (although I scarcely thought it worthy of being credited) so far as to quote it in a lecture he gave at Glasgow some time ago. It described a ball of lightning coming in at the window, running about amongst the people, and brushing up against their legs like a cat, and after that going out by the chimney. This slow discharge by ball lightning has been described very graphically by many people. I never saw any description so minute as that of Arago's, and I must repeat that I scarcely believe it. It was a tailor who reported it, and I think he must have been an imaginative tailor, and let his imagination run away with him when he described a ball of lightning rubbing up against his legs as if it had been a cat purring. In respect to duration, what Mr. Abercromby has called attention to is probably the true explanation. When people see a ball, as it were, coming in by the door, passing along the floor, going up the wall and out of the window, I believe it is altogether a physiological fact. They have been looking in some direction or the other when the flash comes. At the instant that the flash comes there is an intense action on the centre of the retina, especially if they chance to see the flash in the sky. Naturally, after such a startling incident all eyes are moved, and each person after seeing the flash looks about to see what has happened; looks along the floor, looks up the wall, or turns his eyes towards the fireplace or towards a window, and a spot of light (the persisting impression on the retina) follows. So that I believe this marvellous ball of lightning can be seen by every person present going out of his own window or any window that he happens to look out of, or going away up the chimney if he chances to turn his eyes towards the fireplace. I believe Mr. Preece is perfectly right in speaking of the multiplicity of flashes of lightning. He has given some unmistakable experimental and observational evidence of it, which agree altogether with what I have myself noticed. The first time I distinctly remember noticing it was in the summer of 1840 at Frankfort, where I had the pleasure of seeing a great many thunderstorms. I then remarked what I do not remember noticing before, triple and quadruple flashes frequently,* but at such short intervals of time that one could not but think they were somehow connected, and yet at certainly long enough intervals of time to allow me quite distinctly to see that they were not one flash, but several flashes. It was impressed upon me then, and has been since, because I have frequently seen similar phenomena since that time. As Mr. Abercromby said, it did not seem to be repetition of flash along one and the same course, but a succession of connected flashes; a sort of breaking down along many lines and all over the place. One flash seems to cause many others. It is something like setting up a set of bricks or toy soldiers; tumble down one of them and there is a commotion all along the line, with a sensible time interval. I think something must be allowed for sound in this case. Light has been spoken of; the light of one flash tends to produce another flash; if there is a flash ready to take place between two knobs, a flash at a short distance will cause it to pass, according to an experiment which Prof. Schüster has shown me, and which has been referred to by other speakers. It seems that the very fine oscillations of light actually put the air into a condition which is more easily disruptive than air which is not agitated by light; but that will not account for the time interval between the different flashes which was spoken of by Mr. Preece, and which I have noticed myself repeatedly. I think there is something in the velocity of sound, the velocity of propagation of elastic disturbances in the air. The first flash does produce certainly a tremendous disturbance in the air, a tremendous disruption of the air probably causing a very perfect vacuum in the place of the flash, and after that a knocking together of the air. I think we can not account for an electric flash through air otherwise than by supposing a crack in the air suddenly filling up and that producing an exceedingly sharp elastic disturbance. It is quite possible that that elastic disturbance, propagated at a rate nearly equal to the ordinary velocity of sound to another place where the air is in a state of high-tension, ready to break down, causes it to break down. Thus it may be that one flash causes considerable numbers of others at intervals of a quarter of a second, or a few quarter-seconds from its initiation. That is a thing that may be experimented upon. Looking at the photographs exhibited by Mr. Abercromby, I notice a great multiplicity of flashes in one of them. Here are three flashes in one photograph. Query, were they simultaneous?

Mr. ABERCROMBY: We have no evidence; the photograph was sent to us without any particulars.

Sir WILLIAM THOMSON: The subject has always been a most interesting one, but as it is now coming within the range of experimental investigation, which could not be before the photographing of flashes of lightning was practised, I think it would be worth while to make experiments distinctly, to ascertain whether such a group of flashes as that is or is not simultaneous.

There have been some very interesting points with regard to the magnetisation of steel needles by a helix in which there are electric oscillations. If I remember right, Riess experimented on the subject some time prior to 1853. Riess found that when a Leyden jar is discharged through an insulated wire wound up in a helix in the ordinary way, steel needles are found sometimes magnetised in the direction that would be expected, from the direction of the main discharge, and sometimes in the contrary direction. I believe, as Lord Rayleigh stated, that experimenters have actually found differences of direction of magnetisation in different individuals of a group or bundle of steel wires thus actuated, or rather thus exposed to this kind of magnetising action. If you have a very powerful current going in one direction, followed by a current of half that strength going in the opposite direction, a current of a quarter the original strength going in the first direction, and so on, then you may expect the needle to be left magnetised by the current going in that one direction, corresponding to the last time that it had magnetising force enough to magnetise or to reverse the magnetisation of the needle. So that I think by repeating Riess's old experiment with the same degree of electro-magnetic inertia in different successive cases but with different degrees of ohmic resistance, and, therefore, different rates of extinction of the oscillation, probably a full investigation might be made.

I am afraid that I am occupying too much time, but there is still what seems to me to be rather an important point, namely, the protection of buildings. It is rather disturbing to find that a lightning rod has so little protecting power as, according to Prof. Lodge, experiments seemed to prove. With reference to the distinction between copper and iron I would ask Mr. Preece whether he had experimental evidence of any superiority of copper. It is an exceedingly difficult question. Mr. Preece has told us that his experience generally is that lightning conductors are efficacious either with copper or iron as their material. There are certainly a great many iron conductors; and I myself do not know any experiment which proves that they are less efficient. But we cannot say anything certain at the present time. Experiment after experiment, as Prof. Lodge himself has pointed out, must be gone through before we can say which it would be safer to recommend for lightning conductors—iron or copper. There is one point upon which iron has greatly the advantage—that is, that it takes a great deal more heat to melt it. Take iron at a given price, and compare the cost of iron and copper; you can get four or five times as great a mass of iron as of copper for the same money. Take also the higher melting point of iron, and you will see that for the same expenditure upon the lightning conductor you will provide for the consumption of a great deal more energy within itself without destroying it by using iron than by using copper. Then the question of self-induction bearing on magnetisation must be considered before we can say for certain that in all circumstances the iron lightning conductor is as safe as copper. One conclusion seems quite sure—namely, that a sheet-iron house, a sheet-iron roof, with sheet iron walls and sheet-iron floor, is the very safest place that we can possibly be in, or that gunpowder can possibly be in, during a thunderstorm. Here I may say something which, perhaps, is absolutely wrong, but I will hazard it. The subject is full of pitfalls; but I will venture to say that the magnetisability of the iron seems in this case rather to prevent the danger—to prevent the current coming into the inside rather than to facilitate its coming into the inside. I think I may safely say this: that within an iron funnel —a vertical funnel, say—and within a copper vertical funnel, the interior will be protected, in the case of the iron funnel, by the magnetic susceptibility of the iron; whereas in the interior of the copper funnel there will be no protection from this cause, although in each case there will be a very potent protection from the conductivity of the metal. However, I think, with what Prof. Lodge has put before us we know quite enough about iron to say that an iron building will be as safe as safe can be—as safe as science can make it. I think that is rather an important thing for powder magazines, and that the rule should be to have no special lightning conductors at all, but iron roof, iron walls, iron floor—wooden boards over the floor naturally to prevent people setting fire to the gunpowder by walking over sheet iron; and a complete roofing of iron. This iron magazine might be placed on a dry granite rock, or it may be placed on wet ground; it may be placed on anything you please, it might be placed on a foundation under water—no matter what the surroundings are, the interior will be safe. I think that is a somewhat important practical conclusion which may safely be drawn from consideration of these electrical oscillations and the experiments regarding them, and the mathematical calculations of which we have heard so much.

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* See Sir W. Thomson's Collected Papers, Vol. I., pp. 536, 548.

Prof. **ROWLANDS**: The question seems to be whether that experiment of Dr. Lodge's actually represents the case of the lightning. I am very much disposed to think that it does not. In the experiment the whole of the circuit between the condensers was a material of copper and iron, whereas in the case of the lightning the greater portion of the flash is in the air, and, therefore, it may be an entirely different phenomenon in the case of the lightning from what it is in the case of Prof. Lodge's experiment. Furthermore, I am not entirely disposed to think that the length of the spark is an index of the resistance of the conductor in that case, for the reason that we do not know what the length of the spark is when there is an oscillating E.M.F. acting on it. I am not disposed to say that the discharge will take place at the first moment. There may be considerable oscillation, and finally the air may be broken down. The oscillations may take place before the spark goes, in which case the length of the spark may not be an index altogether of what we may call the temporary resistance of the conductor to the passage of the spark. The presence of iron may change the time of oscillation in the same way, so that it may give a different length of spark in that case from what it does in the copper. But I think the fact should be remembered that in the case of lightning the greater proportion of the spark is in the air, which is a very bad conductor, and that therefore the passage of the lightning may not be oscillatory but may be of the nature of swing, in which case a solid conductor or a conductor of copper would probably be better than a tube of iron; but in Prof. Lodge's experiment the whole of the canductors is between the plates of the condenser. With regard to the photographs, I noticed many curious phenomena which I think should be guarded against. I am happy to say that the best one, and which may form the text of my remarks on the rest of them, is an American photograph. In this we have the whole plate exposed and the lightning in the centre of it. I do not suppose that all those were taken at once, but as Sir William Thomson has remarked the plate must have been exposed some time, and have had several discharges upon it. But as I look at this I notice that near the centre the line of discharge is perfectly sharp. As I wander off towards the edge, I see some phenomena which are very similar to these that I see upon these other photographs, which therefore I say is due to the astigmatism of the lens in some way. As you wander from the centre the image is blurred, and you see a number of hazy effects around the spark which may very well be due to the astigmatism of the lens. For instance, some of the sparks there are of this nature.

M. **DE FONVIELLE** (who spoke in French) observed that Sir William Thomson had said most eloquently that Mr. Preece was taking the English side of the question and Dr. Lodge the American side, but he must say that Sir William Thomson himself had taken the French side, and he had proposed a revolutionary system which consisted in the building of iron houses. He took the liberty, though being a Frenchman, to disagree with the great electrician, and to stand with Mr. Preece as an English conservative, with reference to lightning conductors. Lord Rayleigh said that mathematicians and physicists should unite together, but he supposed Lord Rayleigh would agree with him in remarking that Mr. Preece was realising that alliance in a very remarkable manner, for on the one hand he dealt with a large number of experiments and observations of natural facts, and on the other hand he introduced statistics, or rather the calculation of probabilities, which was one of the highest branches of mathematics. The experiments done in laboratories were different from those which were presented by nature only so far as they were conducted on very widely different scales. He congratulated them on the aid they were now receiving from photography. He should advise the meeting to delay its opinion for a time until the completion in Paris of the Eiffel Tower, which would be the most extraordinary lightning conductor in existence, being 1,000 feet high, and which would supply unprecedented observation and experiments. He must, moreover, state that Paris was practically free from calamities produced by lightning. They had erected a sufficient number of lightning rods, according to the principles so admirably advocated by Mr. Preece, and that was a strong evidence that Mr. Preece was altogether travelling in the right direction, quite irrespective of any mathematical or physical demonstration.

Prof. **GEORGE FORBES** said: A question has been raised as between engineers and mathematicians, and some people are apt to take a different view as to what Mr. Preece meant from that which he did mean. I think no man would be more likely than Mr. Preece to mean this—that when a mathematician starts from a number of data and works out mathematically a conclusion, that conclusion is absolutely right and cannot be gainsaid; but what Mr. Preece probably meant to say was that the mathematician might happen to take insufficient data for his premises, and so might eventually arrive at giving advice which was not the best advice which could be given. I think that was all he meant. I think Mr. Preece would admit most readily that the conclusions of a mathematician are absolutely correct, and cannot be wrong; but it is in the choice of the data that he may make a mistake. It reminds me rather of Lord Westbury's advice to a young lawyer. He is reported to have said: "In arguing a case never deduce wrong conclusions from your premises; the facts are at your disposal." Let us keep before our minds in this discussion what the question is which we wish to determine. The question has arisen from Prof. Lodge having come with these experiments to prove that the views of the Committee on Lightning Rods are erroneous. The Committee on Lightning Rods have come to a definite conclusion to make a recommendation that copper should be used for the lightning rods, and Prof. Lodge has come to say that if iron is not better it certainly is as good, and that is the question which has to be decided. Now, Prof. Rowland has already said that it is not quite certain that the experiments which Prof. Lodge has brought forward are perfectly conclusive on this point. No one can fail to be deeply impressed with the beauty and the value of those experiments from a scientific point of view, but

I think it is quite a fair position for Mr. Preece to take up to say that at present they have not proved the fact that the conclusions of the Committee on Lightning Rods ought to be given up. I would illustrate what I mean in this way: We have heard one of Prof. Lodge's experiments very clearly put before us, in which we have an alternative path either of copper or of iron. Previous to those experiments most persons would have expected that copper would have been the better conductor for the alternative path. Prof. Lodge assures us, as the result of his experiments, that copper is not the best and probably is the worse of the two. He has not, however, given us an explanation of this fact, and has not told us why it is so.

Prof. LODGE: I did in the section on Friday.

Prof. FORBES: I beg your pardon. I was going to say that until we heard the explanation of it we should not be in a position to see whether it extended to a flash from the clouds or not. Now I wish to show you the reason why I think we are not fully able to accept this experiment as sufficing to abolish copper rods for lightning conductors. It is this: In the experiment Prof. Lodge has used two condensers, and he has used a special case in which there are two sparks produced beside the alternative path. I would ask Prof. Lodge whether he has tried the experiment in a much simpler way, and more resembling the conditions which exist in the natural phenomena. Supposing, instead of using those two condensers you use the two poles of the Holtz machine; or, instead of having two pairs of knobs, suppose you use only one pair of knobs. You may connect the poles of the Holtz machine with a large battery of Leyden jars, and you will get all the better effect. The arrangement is that you get a spark between the two knobs of the Holtz machine. You put an alternative path in the course of the spark, and then test to see at what distance a spark is just possible to take place. I want to ask Prof. Lodge when that experiment is being performed whether an iron alternative path or a copper alternative path is best. The circumstances seem to me to more resemble the circumstances in nature than the arrangement he has described in his experiment, and I think the conclusion is that the copper would be the best, which would be an argument in favour of Mr. Preece's view.

(*To be concluded.*)

JOINTING UNDERGROUND CABLES.

THE interesting article in the ELECTRICAL REVIEW of 28th September on Joints in Underground Wires, and the attention which is now being directed to the laying of underground mains, induce me to offer a few remarks on the method of jointing some lead-cased underground cables that were laid in Scotland three years ago.

Since they were put down they have required no attention whatever. The tests taken on completion of laying with branch cables and lamp circuits joined up gave a minimum dielectric resistance of 4,300 megohms after three seconds' electrification, which demonstrated that the cables were practically in the same condition as when they were sent from the factory.

Precautions were taken on account of the prevailing wet and boisterous weather to prevent dampness entering the insulating material, and in every case before a joint was commenced to be made, short pieces of cable were cut from the ends of each section to be joined, and these were tested till no deflection could be detected on a galvanometer having a constant of 70,000 w. for one division of the scale.

The lead sheathing and insulating material of the cable were cut back one or two inches to lay bare the conductor; the latter was then bound with fine copper wire, and the ends soldered and squared up or scarfed. A small piece of turned copper pipe of the exact diameter of the conductor was prepared, about 1½ inches long, and sawn through longitudinally on one side; into this split tube was forced the two ends of the conductors till they butted against or wedged each other in the centre of the former, solder was placed over the groove and the application of heat ran the whole together. The joints thus made were found to possess the requisite mechanical strength, and to have an increased conduction varying with the thickness of the split tube.

An adaptation of this method was made for connecting branches to the mains by forming a piece of copper sheet into a hollow T piece, into the cross part of which the main conductor was placed, and the branch was driven home into the vertical portion when solder was applied.

The splicing of the strands was found to take a long time, and to require much skill without commensurate advantage.

The insulating of the joint was proceeded with in the usual manner, but as an extra precaution the insulating mixture was boiled in the pipe (after the latter had been soldered in its place over the joint) and the vent immediately closed.

I found this method of making joints very expeditious and simple, requiring only ordinary care in soldering. I may add that the chief factors of success and subsequent economy consequent on the heavy expense of repairs in laying an efficient system of underground conductors, whether lead-cased or drawn into conduits, are close supervision and excellence of work. Cheap jobs will prove very costly in underground electrical mains.

E. W. BECKINGSALE.

3rd October, 1888.

NOTES.

The Electric Light for Haustholm Lighthouse.—On view at the Copenhagen Exhibition, which is now a thing of the past, was a very interesting set of steam engines, magneto-electric machinery with lamp, lens, &c., complete, for the installation of a powerful electric light at the Haustholm lighthouse, on the west coast of Jutland. There are two magneto-electric machines of the De Meritens type, worked by means of two high and low pressure steam engines. These latter are capable of indicating up to 40 H.P. each, and made to supply the power requisite for driving some air pumps, the function of which is to compress the air for two automatic fog horns. When required to drive the magneto machines only, the engines are merely worked up to about 12 H.P., and while the latter make about 60, the former are driven at the rate of about 850 revolutions per minute. The diameter of the carbons in the arc lamp of the lantern can be varied at pleasure, according to the state of the atmosphere and the light required. The largest size carbons have a diameter of 40 mm.; and with a current equal to 200 ampères and an electromotive force of 42 volts, the candle-power emitted from the arc is about 20,000, which by means of a system of revolving lenses can be concentrated into enormous flashes of light, each flash representing about 5 million candles; the flashes are sent three and three in succession every 10 seconds. When the atmosphere is very clear and transparent the arc lamp is replaced by an incandescent lamp of the "Sunbeam" pattern, the light of which is weaker than that emitted by the arc of the smallest size carbons, with a diameter of 16 mm. only. The arc lamp is of the "Le Baron" type, and the adjustment of the carbons forming the arc is effected in a manner similar to that of all differential lamps. Every four or five hours the carbons have to be renewed, and in order to perform this operation without interfering with the light more than necessary, another lamp is kept in reserve, which can be put into position nearly instantaneously by means of a special contrivance, so that the change is scarcely perceptible from the sea, especially as the light is a flash-light. The beam of light from this lighthouse is visible 27 miles, and the improvement thus afforded to mariners about to make this prominent headland of the Danish coast will no doubt prove of the greatest value.

Electric Lighting in the Navy.—A successful trial of the electric search and incandescent lights has just been made on board the cruiser *Cleopatra* at Keyham.

The electric light recently introduced on her Majesty's naval cadet training ship *Britannia* at Dartmouth, gives much satisfaction. There are, throughout both the *Britannia* and the *Hindostan*, 460 incandescent lamps, each of 16 candle-power. The machinery is located in the after end of the *Hindostan*. The dynamo is a Siemens, giving 400 ampères at 80 volts and 400 revolutions. It is driven by a Willans and Robinson's patent high speed compound engine of 50 horse-power, the large boiler, one of the locomotive tubular type, being from the works of Messrs. Hornsby, Grantham. For night lighting, Elwell and Parker accumulators are used. It is intended to provide shortly a duplicate dynamo for use in the event of an emergency. The large cadet's dining room is lit with 33 lamps, and there is an average of five or six lights in the 17 study rooms. Captain Bedford, C.B., has shown great interest in the introduction of the electric light on the ship. The total cost of the installation has been upwards of £3,000.

Electric Lighting at Monte Carlo.—It is said that the electric light will supersede gas at Monte Carlo this winter, both in the public thoroughfares and inside the Casino.

Lambeth and the Electric Light.—At the last meeting of the Lambeth Vestry a letter was read from Messrs. Bircham and Co., solicitors to the Metropolitan Electric Supply Company, Limited, giving notice of the intention of the company to apply to the Board of Trade under the Electric Lighting Acts, 1882 and 1888, for a license to supply electricity for lighting and other purposes within the area over which the vestry have jurisdiction, and asking for the consent of the vestry to the application. Mr. Ponsford moved, and Mr. J. Hill seconded, that the application be referred to the Electric Lighting Committee.

Dublin and the Electric Light.—A special meeting of the Dublin Municipal Council is announced for Monday, October 22nd, "to consider and do, or cause or direct all necessary acts to be done, upon or in relation to the after-mentioned business—namely, to consider, and, if deemed expedient, to adopt a resolution authorising an application by the Corporation, as local authority for the City of Dublin, to the Board of Trade, for a provisional order to authorise the Corporation as such authority to supply electricity for any public or private purposes within the area of the City of Dublin."

St. George's and the Electric Light.—At the last meeting of the St. George's, Hanover Square, Vestry, the Clerk submitted letters, one from Messrs. Deacon, Gibson, and Medcalf on behalf of the London Electric Supply Corporation, Limited, stating that they desired to apply to the Board of Trade for a licence to authorise them to supply electricity in this parish, and submitting copy of the draft licence; the other from Messrs. Bircham and Co., on behalf of the Metropolitan Electric Supply Company, Limited, stating that it is intended to apply to the Board of Trade for a licence to supply electricity for lighting and other purposes within the parish, and submitting a form of licence. The Clerk remarked that the question of electric lighting had been before the Commissioners of Works. Mr. Southon said this was a question of such a large and delicate character that he did not think they were prepared to go into it at the present moment. He proposed that the receipt of the letter be acknowledged. It would bear further consideration. Mr. Dovey seconded, and the proposal was adopted.

The Electric Light at the Avenue Theatre.—The *Daily News* on Wednesday in a notice of the play at this theatre, said:—"The electric light has now been placed all over the Avenue Theatre, the complete installation on the stage and in the auditorium exceeding a thousand lamps. For a night or two, however, it cannot be used, because the British workmen employed at the works of the Electric Supply Company are indulging in the diversion of a 'strike.' Matters will, it is however expected, be adjusted in a day or two, when the Avenue, which has been redecorated and upholstered, will be one of the prettiest theatres in London."

The Queen and the Electric Light.—The Queen is credited with a hatred of innovations, and the electric light is said to be a particular abomination to her.

Electric Light in a Brewery.—Messrs. Arnold, Perrett and Co., the brewers of the Wickwar ales, so celebrated in the West of England, lit their extensive premises by electricity on the 2nd inst. for the first time. There is nothing very striking about the installation so far as their own buildings are concerned, except that the whole motive power is derived from a huge water-wheel 36 feet in diameter. The dynamo is one of Messrs. Elwell-Parker's shunt wound, and gives 200 volts E.M.F. All the lights in the brewery are 16 C.P. The feature to be most observed is that a circuit has been led from the brewery to the town. This is also of Swan's incandescent lamps, but of 32 C.P., and the wires have been carried bare on poles. The lamps are arranged two in series, and the furthest is rather more than three-quarters of a mile from the machine. Ladies living opposite the lamps say they could easily sit in their windows and see to read or work with no light in their own rooms, although in some cases living on the opposite side of the way. The whole has been carried out by Mr. F. Graham Ansell, formerly of London, the electrician and chemist to the brewery, assisted by the local tradespeople and the brewery men. The inhabitants of the town are delighted

The Telephone in the Sheffield District.—The National Telephone Company, we learn, is just completing its line of communication with Nottingham, Derby, Burton, and Birmingham, and on the occasion of the ironmasters' quarterly meeting in Birmingham on the 11th of this month a call box will be opened in the General Exchange, Birmingham, which will enable members from Sheffield to obtain connection with their works and offices. By an arrangement which is being made with the Northern District Telephone Company communication will be given between Sheffield and Newcastle and the Tyneside towns.

Millions of Telephone Messages.—Over their lines in the West of England and South Wales during the year from September 1st, 1887, to August 31st, 1888, the Western Counties and South Wales Telephone Company carried 3,615,110 telephone messages.

Anglo-American Cables.—We are asked by Mr. H. Weaver, managing director of the Anglo-American Telegraph Company, to publish the following:—"I beg to inform you that the cable laid in 1869, which was broken at 135 miles from St. Pierre in shallow water on the 23rd ultimo, was successfully repaired on the 30th by the Company's ss. *Minia*. A statement having appeared in many of the London papers, to the effect that an entirely new cable could have been laid for the amount which has been expended in its repair and renewal, I should be much obliged if you will kindly correct this statement by publishing the following facts:—The money expended in the repair and renewal of the Brest-St. Pierre cable laid in 1869 (during 19 years) does not amount to one-third of the capital necessary to lay a new cable, even at the present low prices, and that more than two-thirds of the entire cable has never been disturbed since the day it was laid."

Presentation to a Telegraphist.—On Saturday evening at a representative meeting held at the General Post Office, Plymouth, the Superintendent of Post Office Telegraphs (Mr. C. E. Collings), on behalf of the Telegraph and Royal Engineering staffs, presented Sergeant T. Fawssett, R.E., with a handsome marble clock on the occasion of his departure for London on promotion to his regimental department. A hope was expressed by several of the speakers that Sergeant Fawssett might soon return to the postal telegraph department in the capacity of superintending engineer.

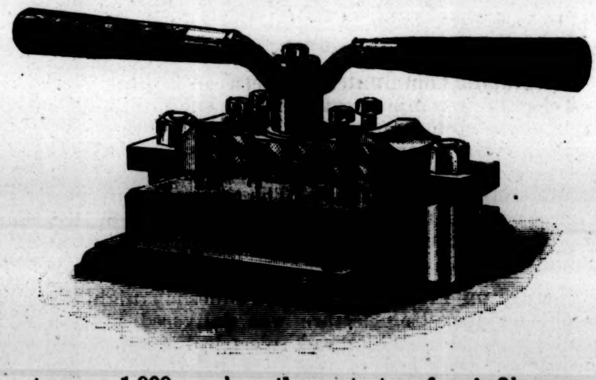
Physical Society of Glasgow University.—At an ordinary meeting of this society, last Friday evening, a paper on "The Electrical Fishes" was read by Prof. M'Kendrick, F.R.S. The lecturer began by mentioning that there were only some 50 known fishes possessing electrical organs, and of these only five or six had been investigated. He then went on to describe the electrical organs of the more important, including the *Raia Torpedo*, or electric ray; the *Gymnotus Electricus*, or Surinam eel; and the *Malapterurus*, or thunder fish of the Arabs. Prof. M'Kendrick particularly called attention to the investigations of the German physiologist, Dr. Max, who spent two years on the Orinoco for the special purpose of studying this subject. The lecturer then concluded by giving a short statement of his own recent investigations.

The Electro-Harmonic Society.—The third season of this musical society will commence on the evening of the 26th inst., at the usual rendezvous, the Banquet Room, St. James's Hall. The concerts will be six in number, two of them being amateur nights, and there is little doubt but that the 1888–89 season will be an eminently successful one. Gentlemen wishing to become members should apply to the hon. secretary at an early date.

Bell's Asbestos.—A new catalogue, with numerous illustrations, is just issued by Bell's Asbestos Company, intended to show the adaptability of the manufactures of the company to every variety of experience, and to enable the steam user to easily find the article best suited to any new difficulty with which he may have to contend. It will be interesting to users of asbestos to learn from the preface that the company is the freeholder of the mines which produce what is recognised as the highest quality of the raw material generally used in the trade, and that the total production of this special quality has for some years been used by this one firm. Not only so, but about one-third of the total quantity of asbestos used in the world is derived from this company's estates. Italian asbestos is used by the company in certain cases where it is found to be preferable, but the teaching of experience is said to be that the asbestos fibre most suitable for the greatest number of applications is that obtained at Thetford, in the province of Quebec. Bell's Asbestos Company was the first to spin asbestos yarn, now so largely used for engine packing and filtering cloth, and had a very interesting demonstration of the process at the Inventions Exhibition of 1885. Asbestos paper, specially prepared for electrical purposes, is coming into extensive use, and asbestos fireproof paints are largely used for the casings and other woodwork in the vicinity of electric light leads. Asbestos paint is also the best insulating and heat-resisting covering for bobbins and dynamos. Asbestos putty being a good insulator, not affected by heat, may be used with advantage instead of vulcanite or ebonite in the construction of arc lamps. The catalogue concludes with a description of asbestoline, and directions for its use on electric lighting and other machinery. For dynamos, we have heard this lubricant highly spoken of, and we see amongst the testimonials one from the manager of the Gülcher Company, who says that after using asbestoline upon the main shafting of the large installation at the Crystal Palace he never had a hot bearing.

The Telephone in Switzerland and Sweden.—Our contemporary, *La Lumière Electrique*, in its issue of the 29th September gives a very detailed account of the Swiss telephone system. The article commences with a comparison between the progress effected in telephonic extension in Switzerland and Sweden. The first named country has a telephone exchange for every 480 inhabitants and the system is entirely in the hands of the State; Sweden possesses a telephone station for every 356 inhabitants, and the telephone lines are almost exclusively constructed and worked by private enterprise. The very general use of this means of communication is due principally to the moderate tariffs charged in these countries, as compared with the rates in France and Germany, and although the telephone in Switzerland is established on an entirely different plan to that in Sweden, the development and the financial success of the two systems present a striking analogy

Patent Adjustable Switches.—These switches have been designed for Messrs. Appleton, Burbey and Williamson, to meet the demand for a reliable and durable switch for electric light and power installations. The ends of the lever are slotted as shown in the illustration, the width of opening being regulated by the set screws. By means of this arrangement thorough contact is obtained, and an entire absence of heating from bad connection. Ample surface is provided: for example, in the switch we illustrate, which is designed to carry 1,000 ampères, the contact surface is 2½ square inches, and they can be made to carry practically any current, the contact surface being increased in proportion. They are mounted on slate or china bases with or without a spring throw off. We have examined several types of these switches, including some smaller kinds with single lever, and they all appear to be made for use and not ornament.



The School of Electrical Engineering and Submarine Telegraphy.—The following gentlemen recently obtained their certificates, as the result of the examination conducted by Mr. H. R. Kempe, of the Engineer-in-Chief's office, G.P.O.:—F. Thomson 99 per cent. of full marks, E. M. Blakely 97 per cent., H. F. Laughton 87 per cent., W. N. Legge 86 per cent., W. W. H. D'Aeth 85 per cent., H. C. Hodges 83 per cent., F. A. Pocklington 82 per cent. No certificate is granted for less than 70 per cent. of full marks, and the candidates had also to satisfy Mr. W. W. Beaumont as to their knowledge of steam engineering. The scheme of instruction of the advanced class has been reorganised under the auspices of the new science instructor, Mr. Chas. Capito, M.I.M.E., &c., with the view of still more completely meeting the requirements of the profession. Some of the above gentlemen have already obtained appointments.

A Sliding Telephone.—A sliding telephone is the latest. It is a movable telephone that can be run up or down to any floor of a large building, so that a person on one story who is wanted at the telephone need not come down or go up to another floor, but can pull the instrument up or down as is required.

Correspondence.—We have received a reply from Mr. Varley to the letter of Prof. Oliver J. Lodge, which appeared in our last issue. Owing to its length and late arrival we are compelled to hold it over till next week.

Electrical Distribution.—Mr. Henry Edmunds has been out of town and has only just had his attention called to the communication of Mr. Johnson regarding electrical distribution. He will reply to the same in our next issue.

Electrical Treatment of Intestinal Occlusion.—At a recent meeting of the Academy of Medicine, says the *Lancet*, Dr. Larat read a note on the treatment by electricity of intestinal occlusion. According to the author, galvanic electrisation of the intestine deserves to be employed in all the cases of occlusion where medical means had failed, and where the obstacle has become insurmountable by the action of purgatives. Moreover, purgatives are useless when the intestine is impermeable, and become injurious in exciting vomiting, which would aggravate still more the condition of the patient. Dr. Larat says that it is essentially of importance not to lose time, and that electricity should be employed as early as possible: in the first place, the procedure would have a better chance of success; and, secondly, if it should fail, surgical intervention would still be possible and have some chance of success. One application of electricity only is often powerless to obtain movement of the bowels; four or five sittings on an average are necessary, and at intervals of several hours. Of 16 cases, Dr. Larat removed the occlusion of the intestine by electrisation in ten.

Telegraphic Communication between Fishing Ports.—Telegraphic communication was opened on Wednesday between Stornoway and the fishing ports of Portnagurran, Gress, and the Port of Ness, in the Isle of Lewis, distant thirteen, nine, and twenty-seven miles respectively from Stornaway. The telegraph will prove a great boon to these districts, and the ports of Ness and Portnagurran will suit admirably as signalling points.

Lecture on Electricity.—On Wednesday night last week an interesting lecture was delivered at Scarborough by Mr. Whitehead, electrical engineer, on electricity. The lecture was of a popular character, and was listened to with evident attention by those present.

University Extension.—Under the auspices of the London Society for the Extension of University Teaching, Mr. W. Lant Carpenter is announced to deliver ten lectures on "Electricity in the Service of Man," at the Chelsea Town Hall, Friday evenings, commencing on October 12th.

Proposed Electric Tramway in the New Forest.—At the last meeting of the Lyndhurst Highway Board a letter was read from Mr. J. C. St. Aubyn Angrove, solicitor, of London, notifying the intention of applying to the Board of Trade for a provisional order sanctioning the construction of an electric tramway in the New Forest between Lyndhurst and the Lyndhurst Road Railway Station. The line would have a 3-foot gauge. The writer requested that the Board might consider the matter at once, as arrangements had been made to proceed with the line immediately the provisional order had been obtained. Several members of the Board expressed strong hostility to the proposal on the ground that it would destroy the glorious scenery of the forest, but the reply was made that to some extent this had already been accomplished by the erection of telephone posts, and that the railway would be run entirely on the "waste," and would not in any way interfere with the road itself. Eventually, however, a resolution was passed to the effect that the Board had a decided objection to allowing the electric tramway to be run in close proximity to horse traffic on the score of danger, and in their opinion the proposal to construct the line within 15 feet of the centre of the road would be wholly inadmissible.

Maxim-Weston Electric Company.—A petition has been presented by Mr. Hugh Watt to the High Court of Justice for the compulsory winding up of this company. The undertaking was established in 1881, with £1 shares, but 15s. per share has since been written off, thus reducing the capital to £76,500. On the Stock Exchange the shares have been virtually unsaleable of late at 2s. 6d.

Finsbury Technical College.—For the Session 1888-9 a special course of evening instruction is announced on Springs, their manufacture and use by engineers, instrument makers and balance makers, by Prof. John Perry, M.E., D.Sc., F.R.S.

Engineering Classes.—The day and evening electrical engineering, laboratory and workshop, magnetism and electricity, steam and applied mechanics classes at the Glasgow and West of Scotland Technical Colleges begin this week. Students whilst studying for the college engineering diplomas never had better opportunities than exist at present of getting suitable employment in the Glasgow engineering works, for the Clyde is very busy and there are many orders on hand.

Tramway Experiments in Birmingham.—According to the Birmingham *Daily Post*, an important experiment, one of a series intended to demonstrate the advantage of electricity over steam as a motor for tramcar propulsion, was made in Birmingham on Monday night and Tuesday morning on the line of the Birmingham Central Tramways Company. The motor, together with the necessary dynamo machine and accumulators, has been made at the works of the Elwell-Parker Engineering Company, Limited, and has already been fully described in our columns. It was arranged to have the first experiment on Monday night as soon as the streets became quiet. About eleven o'clock the car, well lighted by electricity, moved slowly out of the Sparkbrook depôt, having for its driver Mr. Joseph Smith, chairman of the board, and for its passengers Messrs. Carruthers Wain (managing director), Oliver Howl (engine builder), Parker (of the Elwell-Parker Company), A. Dickinson, and a few other privileged gentlemen. Before a hundred yards of the Stratford Road had been covered, a little exciting episode occurred. One of the Corporation carts was standing unattended, when the horse attached to it swerved round and bolted right in front of the bogie engine. A collision followed, but fortunately no harm was done. Mr. Smith applied the brake, and the car was brought to a standstill within a few feet, while someone caught the horse's head. The party then proceeded down Bradford Street, the riding being extremely smooth and pleasant, and no hitch took place until the bottom of the hill was reached. Then, as the car was brought suddenly to a standstill, a snap was heard, and it was discovered that one of the connecting or driving chains had given way. An attempt was made to drive along with the one chain left, but this proved unequal to the strain, and in a few minutes the links gave way. After some discussion as to what was to be done, a steam engine was sent for, and the car and bogie engine were taken back to the depôt. Here there was a delay of about an hour while the chains were repaired. At 1 o'clock this morning a fresh start was made, the intention being to get round to Snow Hill, where the really vital test would be made. But the experimentalists were again doomed to disappointment. When entering Bordesley Street the chains again gave way, and in the early hours of this morning the members of the party had to disperse under circumstances which were not very encouraging, and in no way improved by a downfall of snow. The breaking of the chains was attributed to an undue strain put upon them in the morning, and in no way proved or tended to prove the failure of electricity as a motor. It will probably be deemed advisable to have a set of stronger chains able to bear a greater tension, and with these provided there is no reason at present to doubt the success of the new departure.

Provincial Central Stations.—We gather that the Anglo-American Brush Electric Light Corporation has in view the establishment of central stations in various towns in the provinces, where the corporation has for some time past been pushing the interests of electric lighting, and the undertaking which is in hand at Bournemouth for the supply of electricity to the inhabitants of that town will be completed with all possible dispatch. We wish the enterprise every success.

City and Guilds of London Institute.—The fourth annual dinner of the Old Students' Association will be held at the Holborn Restaurant on Saturday, October 13th, at 7 p.m.

OFFICIAL RETURNS OF ELECTRICAL COMPANIES.

Telephone Union, Limited.—The statutory return of this company, made up to the 11th ult., was filed on the 29th ult. The nominal capital is £10,000,000 in £5 shares, but the only shares at present taken up are the seven subscribed for by the signatories to the memorandum and articles of association, and upon these no call has been made, nor has any amount been paid thereon.

United Kingdom Telephone Union, Limited.—The statutory return of this company, made up to the 11th ult., was filed on the 29th ult. The nominal capital is £10,000,000 in £5 shares; 13 shares have been taken up, but no call has been made, nor has payment been received by the company in respect thereof.

Sudworth & Co., Limited (Electricians).—The first return of this company, made up the 27th June, was filed on the 28th July. The nominal capital is £5,000 divided into 4,725 preferred and 275 deferred shares of £1 each. The shares taken up are 2,031 preference and 275 deferred, upon which the full amount has been called, and of these 2,000 are considered as paid up. The sum of £306 has been received in respect of calls. Registered office, Falcon Works, Bentham Road, South Hackney.

At an extraordinary meeting of the company held at 27, Chancery Lane, on the 1st August, it was proved to the satisfaction of the meeting that, by reason of its liabilities, the company could not continue its business, and it was decided to wind up voluntarily, Mr. James John Deller, of 33, King Street, Cheapside, being appointed liquidator. A notice of this meeting has been duly registered.

St. James and Pall Mall Electric Light Company, Limited—The statutory return of this company, made up to the 13th July, was filed on the 18th July. The nominal capital is £100,000 divided into 19,980 shares of £5 each and 100 shares of £1 each; of the former 5,103 shares have been taken up and £3 per share has been called thereon, and the whole of the £1 shares have been issued and the full amount has been called. The calls paid amount to £13,669 and unpaid to £1,740. Registered office, 6, Westminster Chambers, Victoria Street.

Exeter Electric Light Company, Limited.—The statutory return of this company, made up to the 21st July, was filed on the 26th July. The nominal capital is £20,000 in £10 shares. 404 shares have been issued and £2 per share has been called, the calls paid amounting to £498 and unpaid to £310. Registered office, 16, Cathedral Yard, Exeter.

NEW COMPANY REGISTERED.

Ward Electrical Car Company, Limited.—Capital £300,000 in £10 shares. Object:—To adopt an agreement of 1st inst. between Michael Radcliffe Ward and Ashton Arthur Mackintosh. To manufacture or acquire vehicles capable of being used in connection with the generation, distribution, supply, accumulation, employment or utilisation of electricity, and generally to carry on the business of electrical and mechanical engineers. Signatories (with one share each): J. W. Oxley, 29, Muriel Street, Barnsbury; Hy. Mayor, 9, Bridgewater Square, E.C.; L. H. Benjamin, 29, Sandringham Road, E.; A. V. W. Capps, 18, Kelvin Road, N.; C. W. Bersey, 10, Larkhall Lane, S.W.; A. W. Wells, Greenhill Park, Harlesden; J. B. Lawson, 30, Cambridge Street, Eccleston Square, S.W. The signatories are to appoint the first directors; qualification, 50 shares. The directors are to fix their own remuneration, which, however, is to be payable out of profits and is not to exceed £2,000 over and above the remuneration of the managing director. Registered 2nd inst. by E. F. B. Harston, 4, Bishopsgate Street, E.C.

CITY NOTES, REPORTS, MEETINGS, &c.

Maxim-Weston Electric Company, Limited.

AN extraordinary general meeting of this company was held at the Cannon Street Hotel on Wednesday, Mr. John Marks presiding, when the following directors' report and accompanying accountant's statement were submitted:—The directors take the earliest opportunity of laying before the shareholders a statement of the position of the company's affairs. Immediately after the election of the new directors on July 19th last, the board met at the company's offices in Queen Victoria Street, and Mr. John Marks was elected chairman. No assistance was rendered to the new directors by the managing director, Mr. Hugh Watt, or by any of the officers of the company, and it soon became clear that it was quite impossible to carry on the business of the company with the then existing officials. The directors were on several occasions unable to obtain admittance to the company's offices, Mr. Wright declined to fulfil his duties as secretary, and the clerks, though paid their salaries out of the funds of the company, declared that they were in the service of Messrs. Hugh Watt & Co. On the 3rd of August last the board decided to have the affairs of the company investigated, and the principal books were accordingly sent to the office of Messrs. Boyce & Ramsay, chartered accountants, No. 2, Metal Exchange Buildings, with instructions to investigate and report to the board. On the same date Mr. Watt caused the books and papers remaining at the office to be sent to the company's works, were they were deposited in confusion amongst the machinery. On the following day (4th August) Mr. Watt handed in his resignation, which was immediately and unanimously accepted. Mr. Wright, having refused to obey instructions, was informed that his services as secretary would no longer be required. Mr. Samuel J. Boyce was then appointed secretary of the company, and in compliance with Mr. Watt's request that the offices should be at once vacated, the registered offices were transferred to No. 2, Metal Exchange Buildings, Gracechurch Street, E.C. The directors now feel that by the exercise of energy and patience the company's affairs have been extricated from the confusion in which they found them, and they are enabled to lay before the shareholders a fairly accurate statement of the position of the company. The directors are also pleased to state that they have already effected a reduction in management expenses of over £1,000 a year. Upon the change of management, instructions were given to the works manager to stop all unnecessary and useless expenditure at the works, and to take the stock at cost price and to give an approximate value of the plant and machinery. This has been completed, and the report of Messrs. Boyce and Ramsay is now in the hands of the directors, and a copy is herewith enclosed. By this report it will be seen that the assets were stated in the balance sheet, at 31st December, 1887, at about £17,445 in excess of their value, and that the results of the trading have not been truly stated in the past. The directors, after examination, have verified the statements contained in Messrs. Boyce and Ramsay's report, and it is for the shareholders to look the facts fairly in the face and come to a speedy decision as to the future of the company. The directors have already made a claim upon Mr. Watt for the repayment of various sums to which they consider the company are entitled. Notwithstanding the present unsatisfactory position of the company, the directors are strongly of opinion that with honest and attentive management, a good and profitable business may be carried on. The company possess valuable patents, and good plant and machinery adapted to the requirements of the business, a state of things only to be attained by years of experience and the expenditure of a large capital. They also have a large and valuable stock. Installations have been erected in various parts of the country, and numerous applications are being made for estimates from all parts of the kingdom, showing that the company's system is appreciated. Under these circumstances, the directors earnestly urge upon the shareholders the desirability of providing sufficient working capital for carrying on the business, as by that means alone can a new and prosperous business be developed. The directors therefore recommend that the capital of the company should be reduced by the cancellation of 2s. 6d. per share upon the existing 5s. shares, and that to provide working capital the 188,000 shares remaining unissued should be issued at 2s. 6d. each. With this object in view the directors have convened an extraordinary general meeting of the shareholders, to be held on Wednesday, the 3rd October, 1888, to which the necessary propositions will be submitted. They propose to issue the new shares of 2s. 6d. each immediately after the confirmation of the proposed resolutions; payable—sixpence on application, sixpence on allotment, and the remainder by instalments of sixpence each at intervals of three, six and nine months.

TO THE DIRECTORS OF THE MAXIM-WESTON ELECTRIC COMPANY, LIMITED.

Gentlemen,—In accordance with your instructions we have investigated the accounts of your company, and beg to report as follows:—

We have taken the balance sheet and profit and loss account for the year ending 31st December, 1887, and have confined our investigation to those accounts, except where we have found it necessary to go back into former years for the purpose of elucidating certain items appearing in the 1887 accounts.

The assets are set forth in the 1887 balance sheet as follows:—.

Patents	£40,737	1	1
Stock of machinery, lamps, &c., at factory	19,817	7	6
Machinery, plant, and tools	3,603	15	2
Furniture and fittings at offices ...	228	1	8
Shares in the Electric Sun Lamp and Power Company Limited, estimated value	239	7	6
Debtors	15,001	12	6
Cash	2,370	11	0
	£81,997	16	5

We will deal with these items in their order.

Patents: £40,737 1s. 1d.—This item appeared in the 1886 balance sheet as £38,229 17s. 1d., showing an increase during the year of £2,507 4s. This increase is arrived at by charging to patents account the following sums:—

Renewal of patents	£44	0	0
Wages	718	4	0
Material used and destroyed casting patterns, &c.	325	0	0
Managing director, proportion of salary ...	750	0	0
Works manager „ „ ...	200	0	0
Secretary's time taken up experimenting on dynamos, lamps, &c.	100	0	0
Clerks' time taken up on experimental work	270	0	0
Men's time	100	0	0
	£2,507	4	0

There being no sinking fund for the gradual extinction of the item for patents, the cost of experimenting such as above should, in our opinion, be paid out of revenue. In our judgment the value of the patents should not be permanently increased by charging to them a large proportion of the salaries of the managing director, works manager, secretary, and clerks, which would have been a charge upon revenue whether experiments had been made or not. The auditor refers to this matter in his report on the 1886 accounts. The amount at which the patents are entered in the books should, in our opinion, be subject to a very heavy depreciation—to the extent of the amount added during 1887, or, say, £2,500.

The amount expended upon the "Watt" dynamo during 1887, as worked out by the works manager, was—

Labour	£153	0	2			
Material	158	9	9			
				£311	9	11
and in 1888						
Labour	129	5	6			
Material	121	10	0			
				250	15	6
				£562	5	5

The amount expended upon the "Watt" lamp was:—

1887.	Labour	£30	12	2			
	Material...	4	1	11½			
					34	14	1½
1888.	Labour	98	17	8			
	Material...	37	15	1½			
					136	12	9½
					£171	6	11

Stock	£19,817	7	6
Machinery, plant, &c.	3,603	15	2
Furniture and fittings	228	1	8
	£23,649	4	4

The stock has recently been re-taken, and the plant and machinery re-valued by the works manager, who puts upon them now a lower value than at 31st December, 1887. Taking his new valuation as the basis it would appear that the value of the stock, plant, machinery, tools, furniture, and fixtures, at 31st December, 1887, was about 14,950, instead of £23,649 as stated in the balance sheet. This shows a difference of, say, £8,700. A comparison of the stock lists will show how this difference is made up.

Debtors: £15,001 12s. 6d.—The good debts owing at that date were, so far as possible, collected prior to the resignation of your late chairman and managing director. A large sum is represented by plant in the hands of agents, some of which has since been returned, and much is reported by the agents to be unsaleable in their districts. A large proportion of the remaining accounts is in dispute. We have gone carefully into all these accounts, and are of opinion that in order to arrive at a fair estimate of their value, a sum of £7,600 should be written off or provided for. The details of this sum we give in a separate list, showing the amount we recommend should be provided for in each case. This item is particularly mentioned in the auditor's report on the accounts to 31st December, 1887.

Shares of the Electric Sun Lamp Company, in Liquidation, £239 7s. 6d.—We can gain no information about this company, and think it doubtful if there will be a further dividend. It would be prudent, therefore, to provide for the amount, or the greater part of it.

Cash, £2,370 11s.—This item is correct.

Applying the foregoing estimate of the assets to the figures given in the balance sheet at December 31st, 1887, we find that, in our opinion, the assets are stated in excess of their value at that date, as follows:—

Patents	say	£2,500	0	0
Stock ... ... Machinery, plant, &c. Furniture ... }	„	8,700	0	0
Debtors	„	7,600	0	0
Shares in Electric Sun Lamp Company	„	200	0	0
Total	„	£19,000	0	0
Deduct balance of profit and loss account	„	1,555	0	0
Total		£17,445	0	0

Profit and Loss Account.—During the years 1884 and 1885 goods were sent out to various agents abroad, and entered up in the company's books as though actually sold to the extent of £5,334 0s. 7d., and the assumed profit in consequence was taken into the profit and loss accounts for those years. A few additional items have been sent out since, bringing up the total to £5,412 10s. 5d.

Of this amount goods to the value of £699 8s. 5d. only have been sold. The remainder is in the hands of the agents unsold, or has been returned into stock.

Mr. Watt has drawn 5 per cent. commission, amounting to £571 17s. 6d., on the alleged profits of the years 1884 and 1885, which includes about £145 on the assumed profits on these alleged sales.

In the directors' report accompanying the accounts for 1885 are the following clauses:—

1. "The directors beg to remind shareholders that no allowance was made during the past two years for contingencies, such as referred to in section E of clause 75 of the articles (which states that the directors may 'set aside *out of the profits of the company* such sums as they think proper to meet contingencies, &c.'), and they have now deemed it prudent to pass the sum of £3,606 5s. 0d. to the credit of a general reserve fund."

2. "After making the above reserve, the total balance to the credit of profit and loss account, including amount brought forward from previous years, is £5,284 19s. 8d."

It would *appear*, therefore, from the foregoing, that the reserve was created out of profits on trading. It was *actually* created in the following manner:—

Premium on new shares issued				£3,000	0	0
Less paid to cover commission on placing shares	£1,000	0	0			
Brokerage	122	18	4			
				1,122	18	4
				1,877	1	8
On the reduction of capital there was a balance on the books to the credit of the company of				1,729	3	4
The two items added together made ...				£3,606	5	0

the amount at which the reserve fund appeared in the 1885 balance sheet.

Clause 6 of the same directors' report is as follows:—

6. "The directors estimate that £600 will cover the total bad debts incurred by the present management, including outstanding debtors."

The "outstanding debtors" amounted to £12,794 15s. 6d., and this sum included the goods in the hands of agents amounting at that date to £4,902 19s. 9d.

During 1886 a sum of £2,074 11s. 5d. is written off the agency accounts alone, and £1,531 13s. 7d. from other accounts. These items together absorb the reserve fund of £3,606 5s., which accordingly does not appear in the 1886 balance sheet. No explanation of its disappearance is given in the directors' report accompanying the accounts for that year, but the auditor in his report on the 1886 accounts draws attention to the fact. The result of this operation was that the profits of the years 1884 and 1885 were swollen by goods sent out to agents at enhanced prices being treated as sales, without any provision being made for bad debts, or for the contingency that the goods might not be sold. The reserve fund—created by reduction of capital and premium on shares—being utilised to reduce the amount appearing as book debts, the appearance of profit was maintained, and a dividend of 7½ per cent. was paid in March, 1886.

A subsequent dividend of 5 per cent. was paid in March, 1887, out of the amount shown as profit in the accounts for the year 1886, no provision being made for loss on book debts during that year.

From the foregoing it will be seen that the results of the trading have not been truly stated in the past by reason of the non-provision for bad debts, and by the over valuation of the stocks.

BOYCE & RAMSAY, *Chartered Accountants.*

2, Metal Exchange Buildings, E.C.
14th September, 1888.

Mr. Samuel J. Boyce, the Secretary, having read the notice convening the meeting,

The Chairman said: The duty now devolves upon me of moving the adoption of the resolution that has been read by the secretary, and which is contained in the notice convening this meeting. But before doing that it will be perhaps as well for us to retrace our steps a little, and devote a few minutes to a consideration of the causes which have led us to the necessity of asking you to consent to a reduction of capital. You will recollect, gentlemen, that in March last the late board called the annual general meeting of the shareholders, and Mr. Watt, the then managing director of the company, having moved that the report and the accounts should be adopted, Mr. Swaby, the only then remaining director with Mr. Watt, moved the rejection of the report and accounts on the ground that they were altogether untrue. This led up to some discussion, and Mr. Watt, I think you will bear in mind, made some very grave charges against Mr. Swaby at that meeting, which it is very satisfactory to us to know have been refuted in the law courts since. As a sort of compromise, a committee was appointed to enquire into the affairs of the company, and to report to the shareholders at a subsequent meeting. That committee did their work as well as they could under the circumstances and with the limited power with which they were invested. The result of their investigation was that they reported to the shareholders that the balance sheet presented by Mr. Watt was an untrue one, and they recommended the shareholders to appoint additional directors to act with Mr. Watt in carrying on the affairs of the company in future. At a subsequent meeting, at which Mr. Watt presided, five gentlemen were named as new directors, and the names were supported from the chair and carried unanimously. That was on July 19th. The new directors at once set themselves to work, and on that very day two of our number called at the office, and requested the secretary to call a board meeting for the following day. Mr. Watt had, however, to preside at a meeting of the New Chile Company, I believe, on the following day, and it was arranged that the board meeting should take place two days later, on July 21st—two days after the election of the new directors. At that board meeting Mr. Watt was not present. The directors asked the secretary to produce the books and documents belonging to the company; but he replied that they were locked up in the safe, and that the key of the safe was in Mr. Watt's possession. We were unable, therefore, to transact any business whatever at that meeting; but we did the best we could under the circumstances—we instructed the secretary to have returns prepared of the employés at the works and the clerks in the offices, with their respective salaries. At that meeting, when we asked the secretary what he had to report, he said that a cheque for £724 had been received from the Peel Mills Company. The cash book was not locked up in the safe, so we looked to see if the cheque was entered up. It was not entered up, and there was no entry in the bank-book.

A Shareholder: When was that?

The Chairman: At the first board meeting, on July 21st.

The Shareholder: When was the cheque received?

The Chairman: Some days previously. Upon asking the reason why the cheque had not been paid in, the secretary informed us that he had endorsed the cheque and entered it, and that Mr. Watt had then paid it into his private account, saying that the company was indebted to him, which was true. Mr. Watt retained the cheque and paid into the company's account a cheque for £74 only, retaining £650 against what he claimed as being due to him from the company. At the following meeting, on July 27th, Mr. Watt was present, but, notwithstanding that he had presided at the meeting at which the new directors, whom he supported, were elected, he positively refused to let us have access to the minute book and the other documents of the company, and which were absolutely necessary for the purpose of carrying on the business of the company, and the ground upon which his refusal was based was that he questioned the validity of our election, and that until he had taken a legal opinion on that point he refused to give us any assistance whatever in carrying on your business. We, however, took the opinion of Mr. Emden, through our solicitors (Messrs. Nashville and Withers), and that was that we were legally appointed directors of the company, and had a right to carry on the business of the company on behalf of the shareholders. The chairman then referred to the steps which had led up to the resignation of Mr. Hugh Watt, and said that Mr. Watt caused all the books and papers to be removed to the works. The then secretary absolutely refused to carry out the instructions of the board, and he was therefore peremptorily dismissed. The Chairman continned: Then came the question of the clerks. Upon inquiring of the clerks whether they were the servants of the company, they said they were not—they were the servants of Messrs. Hugh Watt and Co. We asked the clerks what salaries they were receiving, and upon their telling us we turned to the books and found that the amounts were paid by this company. The whole staff of clerks paid by the company were clerks of Messrs. Hugh Watt & Co. They were doing the clerical work of the Maxim-Weston Company the Yuruari Mining Company, the New Chile Mining Company, and the Port Darwen Company, and there was a shorthand clerk doing Mr. Watt's correspondence with his constituents in Glasgow. On August 3rd, the day before his resignation, Mr. Watt called in a firm of engineers in the City, took them to the works and sold them some movable plant and stock for a sum of £175; and two engines alone, part of this property, had cost the company between £300 and £400, and the very morning of the day on which he resigned he collected the money, and paid it into his own private account. Referring to Messrs. Boyce and Ramsay's investigation of the books, you will remember that in the annual balance sheet the profit on the year's trading was put down at £1,555. As to how Mr. Watt dealt with some of these items I have the particulars before me. First, as to the Palace Hotel, Birkdale. A question was asked about the debtors at the meeting in March last, and Mr. Watt said that the whole of these debts were "good and recoverable." I am quoting his own words. The Palace Hotel, Birkdale, was indebted to the company in the sum of £2,750 for plant supplied. That company went into liquidation on November 30th, 1887—that is to say, a full month before the balance sheet was made up, and about four months previous to Mr. Watt's statement that all the debts were good and recoverable. I say on November 30th, 1887, that company went into liquidation. We took back our lighting plant in discharge of our debt of £2,725, and Mr. Watt told you in March last that the debts were good and recoverable, knowing that this company, owing £2,700, was in liquidation. Lewis & Co., of Manchester, by our books were owing £1,265, of which they never paid a penny piece, and in June of this year the plant was returned to the company, and we sustained a loss of something like £700. Plant to the value of £5,000 or £6,000 has been consigned to various parties abroad—to some people in the Mauritius and in various parts of the colonies. That has been written in the books as if it were actual sales. A profit amounting to upwards of £2,000 is assumed to have been made, and upon this £2,000 of course Mr. Watt has taken his commission of 5 per cent. When we came to correspond with those gentlemen in the Mauritius they informed us that they have sold between £500 and £600, and they will be very pleased to return the remainder. And this is what Mr. Watt treats as a bona-fide sale according to your books, and upon which he takes his commission as managing director! I could go on *ad infinitum* with similar instances, but I think I have now shown you sufficiently how Mr. Watt has made up his accounts, and how it is that he has credited you with a profit of £1,555 on your last year's trading.

A Shareholder: You have other instances?

The Chairman: I am sorry to say we have. I think I have said sufficient to show you the manner in which Mr. Watt has mismanaged the company, and for which he was paid the large salary of £1,200 a year and 5 per cent. commission. So much for the past; now for the present. Let us turn to the directors' report. You will find in one paragraph of the report the directors say the company possess valuable patents and good plant and machinery adapted to the requirements of the business—a state of things only to be obtained by years of experience and the expenditure of large capital. They also have a large and valuable stock. Well gentlemen, we consider that after writing off the amounts recommended by the accountants, after reducing the value of the patents, the present assets of the company represent about 2s. 6d. per share. We therefore propose to start *de novo* with the reduction of the capital from 5s. to 2s. 6d., and we ask you shareholders to take up the unissued shares, amounting to 188,000, at 2s. 6d. The directors are prepared to do their share in taking up these shares, and we have applications from gentlemen who are unable to attend this meeting to the extent of some thousands of the new shares. Now, gentlemen, we know perfectly well that we labour under this disadvantage, that you can purchase these shares in the market at less than 2s. 6d. But we feel confident, as stated in the report, that, notwithstanding the present unsatisfactory position of the company, with honest and attentive management a good and profitable business may be carried on, and therefore we strongly recommend you to fall into the scheme and reduce the capital to 2s. 6d. and take up these unissued shares, because we feel confident with a capital of £23,500, which it will give us, as a working capital the present shares, which can be bought at considerably less, will be brought up to more than their present market value. The Chairman then proceeded to answer seriatim the statements and questions in a circular issued that morning by Mr. Watt; he characterised the circular as "a tissue of falsehoods," and "downright lies," and said some of the insinuations which it contained were exceedingly mean. A representative of the parent company had been on a visit to this country for some months past, and he had heard something of the affairs of the company. He thought at first that the state of the company was due to the want of knowledge on the part of the works manager, but he made enquiries and got to know all about it; he found out the feeling which existed between Mr. Hugh Watt and the shareholders, and the feeling existing generally with regard to Mr. Watt. While Mr. Watt remained at the head of affairs he resolved to have no communication with the Maxim-Weston Company. When the change came he asked to be allowed to meet the board, and did meet them. He told them that many improvements had taken place in the Maxim-Weston patents, which they ought to be in possession of, and he advised them that if they sent one member of the board, together with the engineer, over to the States, it would be greatly to the benefit of the company. He also pointed out that a great reduction had been made in the price of the lamps, so that by having the new lamps they would be in a position to sell, at a considerably less price, better lamps than those sold by competing companies. The representative of the parent company further suggested, although he had no definite instructions, that it was highly probable that the American company would assist them in the matter of new capital.

After further recommending the scheme proposed in the report of the directors, the Chairman concluded by moving the following resolution :—"That the capital of the company be reduced from £125,000, divided in 500,000 shares of 5s. each, to £62,500, divided into 500,000 shares of 2s. 6d. each, and that such reduction be effected by cancelling capital which has been lost or is unrepresented by available assets to the extent of 2s. 6d. per share upon each of the 312,000 shares which have been issued and are now outstanding, and by reducing the nominal amount of all the shares in the company's capital from 5s. to 2s. 6d. per share."

Mr. J. M. Klenck seconded the motion.

Mr. Dever, the auditor, with the view of repudiating the suggestion in Mr. Watt's circular that he had passed the accounts of previous years to which exception was now taken, read letters which he had sent with his signature of the accounts, in which letters he drew attention in the balance sheet by stating "audited and found correct with my letter." Those letters, he said, if read, as they should have been, to the shareholders, would have caused them to make enquiries which would have led to the discovery of the irregularities which had taken place.

Messrs. Empson, Griffiths, Newton, and others having spoken, the resolution was put and carried unanimously.

Mr. Hirsch then moved: "That the directors of this company be, and are, hereby authorised to oppose Mr. Hugh Watt's petition for winding up this company." And, secondly: "That the directors of this company be, and are, hereby authorised to take immediate legal steps against Mr. Hugh Watt for the purpose of recovering such sum or sums as are due by him, the said Mr. Hugh Watt, to the company."

After some discussion the first resolution was carried unanimously, and the second with two dissentients.

Mr. Hildyard, in reply to a question, said that, so far as the board had been able to ascertain, Mr. Watt had spent about 60 hours at the factory during 1887; his pay had therefore been at the rate of about £20 an hour.

The meeting then closed with a vote of thanks to the chairman and directors.

The Direct Spanish Telegraph Company, Limited.

THE thirtieth ordinary general meeting of this company was held at Winchester House, Old Broad Street, on Tuesday, Sir James Anderson presiding.

Mr. Ch. Gerhardi, the secretary, having read the notice convening the meeting,

The Chairman, in moving the adoption of the report, said that on the last occasion he held out the hope that they would soon reach a dividend of 5 per cent. He was happy to say they had reached that point. During the three months that had elapsed since the issue of the report they had carried 2,000 more messages than in the corresponding quarter of last year. Still, they had not made any more profit, by reason of the growing habit of codifying messages, and the influence upon the traffic they received, by the raising of the tariff on the American lines, affecting the European traffic from which they got the most money. However, the business was sound and commercial, there being nothing political or spasmodic about it. Everything was progressing satisfactorily, and the cables were in good order. Considering that there had been the increase of 2,000 messages in the dull three months, it augured well for the quarter that followed. There had certainly been an increase of £851 in the receipts, but to obtain that they had to spend an additional sum of £260.

Mr. E. Etlinger seconded the adoption of the report, which was carried unanimously. A vote of thanks to the chairman and directors thereupon terminated the meeting.

The Eastern Extension, Australasia and China Telegraph Company, Limited.

THE report of the Eastern Extension Telegraph Company for the past half-year shows that the receipts, including Government subsidies, were £239,926. The working expenses amounted to £69,297; interest on debentures, sinking fund requirements, &c., absorbed £44 8s., and the balance of cost of the Singapore-Saigon renewal was £15,401. After deducting these items an available balance of £110,403 is left, which allows of a quarter's dividend of 1¼ per cent. being paid, and £47,903 being carried forward. In consequence of the serious inconvenience to the telegraphing community occasioned by the recent unfortunate interruptions, and in order to avert a recurrence of this as far as possible, the directors have decided to lay an additional cable from Banjoewangie to Western Australia, to connect with the land lines of that colony. This line will give the colonies the advantage of a third cable to Java, and an alternative land system *viâ* Perth and Eucla, to the existing overland line between Port Darwin and Adelaide.

TRAFFIC RECEIPTS.

The Western and Brazilian Telegraph Company, Limited. The receipts for the week ending September 28th, 1888, after deducting the fifth of the gross receipts payable to the London Platino-Brazilian Telegraph Company, Limited, were £3,947.

The Cuba Submarine Telegraph Company, Limited. The number of messages passing over the lines of this company during the month of September was 3,550, estimated to produce £2,700, against 3,069 messages, producing £2,494 in the corresponding month of last year.

The West India and Panama Telegraph Company, Limited. The estimated receipts for the half-month ended the 30th September are £2,034, as compared with £2,388 in the corresponding period of 1887.

The Direct Spanish Telegraph Company, Limited. The estimated receipts for the month of September were £1,888, against £1,996 in the corresponding period of last year.

The West Coast of America Telegraph Company, Limited. The gross earnings for the month ending September 30th are £4,875.

THE BRITISH ASSOCIATION.

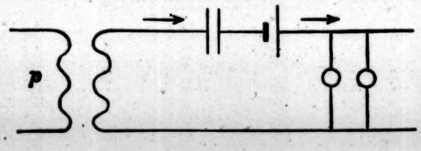
ELECTRIC LIGHTING SYSTEMS.

(*Section G.—Mechanical Science.—Monday, September 10th.*)

In consequence of pressure of time three papers on electric lighting subjects were discussed together, namely the paper of Prof. Geo. Forbes on "Electric Lighting in America," and that of Mr. Henry Edmunds on "A system of Electrical Distribution," both of which appeared in the REVIEW for September 14th, and the paper of Mr. W. Lowrie on "The Measurement of Electricity in a House-to-House Supply," published in the REVIEW for September 21st.

DISCUSSION.

The **PRESIDENT** (Mr. Preece), referring to Mr. Lowrie's paper, which was the last read, said that although the system it described had been in practical use for some time at Eastbourne, it had been kept very quiet, and he believed that was the first time it had been published. He was afraid it had been rather difficult to derive information from the paper as to the principle on which it worked, but he would put on the board the impression conveyed to his mind when he saw it. It appeared from all the diagrams that they could show the arrangement in a house worked on the secondary generator principle by the two parallel lines of the diagram. There was a constant exchange of positive and negative currents. If they were to put into the secondary circuits a decomposing cell there would be no effect whatever, because whatever the positive current did would be wiped out instantly by the exactly equivalent opposite current that followed. What Mr. Lowrie had done, and what appeared to be the secret of the meter, was that he added a secondary cell, and that introduced into the circuit a constant E.M.F., tending to send a weak current in the direction of the arrows, and through the cell in that direction. When no lamps were in circuit no current could flow, and it could only flow during the time the secondary cell was in action. This constant E.M.F. of 2 volts always added to the E.M.F. of the working currents in one direction and opposed the working E.M.F. in the other direction, and the result evidently was that they got the copper, or whatever it might be, deposited in the same proportion as they would if the constant E.M.F. was only at work during the time the lamps were alight. It appeared that the meter was acting with the alternating system, and recorded the time the lamps were at work by the weight of copper electrodeposited. It seemed to him to be the basis of something that contained a little accuracy in it, and it was certainly a novelty. They had heard two other admirable papers by Prof. Forbes and Mr. Edmunds, and he hoped all three would be adequately discussed.



SIR WILLIAM THOMSON said he had heard with great interest of the advance in electric lighting in America. What took place in that country was only partially known here, but the more they heard of it the more they admired and held it up as a standard which they ought to reach and surpass in this country. They certainly had been slow in attaining such results as had been achieved. The great question of to-day as regards electric lighting was, Which of the two kinds is to be—the alternating current or transformer system, or the constant current system. There was one way of carrying out the direct current system, and which they had not heard at all, unless it was mentioned in Mr. Edmunds's paper, and that was by the use of accumulators, so as to allow small wires of high potential to be used for carrying current to a distance and to distribute the supply by subdividing the accumulators. That he believed, on second thoughts, had been described by Mr. Edmunds, and it was one of the most important parts of the subject. He did not think it was possible for them to decide which was the better system. Possibly one might prevail for a time and the other ultimately. There were important economic questions to be decided. There was the great cost of the accumulators, for instance, which must be considered. If it were not for the cost of the accumulators he thought they might all say that the direct system had great advantages, and might be expected to prevail over the alternating transformers. One great point with the latter was the difficulty of applying machines in parallel to work together. Some who had gone into the question had come to the conclusion that very large dynamos, giving the whole amount of work should be used, and for less work one smaller dynamo Prof. Forbes had described. He (Sir William) would ask, had that been worked out in practice so as to give really satisfactory results? How about the dynamos keeping absolute time if they were joined by belts and not coupled rigidly together on the one axis? There was another subject to be considered: Given two or three or more alternate current dynamos rigidly connected with one belt, the question was, would an electric current stay quiet and distribute itself equally between the different dynamos? That was a question that could be answered theoretically, but they wanted to hear something of practical results, Prof. Forbes spoke of different dynamos not coupled together rigidly, but each driven by a belt. Did the tendency to keep together in those cases, when once they got within a certain narrow limit of being exactly in time, suffice to keep them together irrespective of slight slips of the belt? He hoped Prof. Forbes could give them some information upon that subject. He could not quite follow with agreement what Prof. Forbes said about the voltage of lamps. To the best of his knowledge a 100-volt lamp was much more desirable than a 50-volt lamp. He knew of no reason for preferring a 50-volt lamp with respect to durability. He had lamps of 86 volts, and should wish to change them for others of 100 volts until he saw reason to change his opinion. The percentage of lamps used up in a year and requiring renewal with 100 volts was so small that the cost of renewals was a very insignificant part of the whole cost of an electric light installation. If it were true, as had been suggested, that 50-volt lamps had greater durability, that would be a strong reason for preferring them, but it would not be a determining reason. In all other respects 100-volt lamps were much more convenient. The quantity of copper used in wiring a private house or public building requiring two or three hundred lamps was very considerable; but it would be just half with 100 volts what it would be with 50 volts. Then the fact that a smaller current would be required was another strong reason in favour of 100 volts. In houses two things had to be considered; the highest for safety and convenience on the one hand, and the highest for economy on the other. At present they were perfectly happy with 200 volts in their houses; and if they could get 200-volt lamps from the lamp-maker nearly equal in durability with the 100-volt lamps, he should strongly prefer them. He would not go below 100 volts. Turning to another question of great practical importance, Sir William said he thought 8,000 alternations would introduce more sources of want of actual economy than it would avoid, but with 5,000 alternations per minute there was only about 3 mm. of the conductors that was useful up to 65 per cent. of the full conductivity of copper. But if they got 3 mm. in from the circuit, if the average current was only 65 per cent., the result was that if they were satisfied with less than 65 per cent. conductivity, they might take it thicker. But it was a sure practical rule that if the alternations were to be at the rate of 5,000 per minute, the conductors should be either thin plates which might be 6 mm. thick or tubes 3 mm. thick. That was a very large question, and pointed to a greater ease of getting copper to do its work with 5,000 than with 8,000 alternations. But there were other advantages of greater frequency and greater number of alternations.

Mr. R. P. Sellon said he felt great hesitation in expressing an opinion differing from that of Sir William Thomsen, but as one practically acquainted with the manufacture of incandescent lamps he thought he might venture to speak on the subject. He thought there was little reason to doubt but that low E.M.F. lamps were longer-lived than high E.M.F. lamps, looking at the question purely from the point of view of the life of the lamp. One reason for this lay in the fact that the smallest flaw in manufacture, the smallest inequality in section, the smallest impurity which existed in the carbon in the initial stage of its manufacture, would amount to a much smaller proportion of the whole cross-sectional area in the case of a low E.M.F. (and consequently comparatively thick) carbon, than in the case of a high E.M.F. (and consequently comparatively thin) carbon. Thus the temperature of the low E.M.F. carbon would be the more uniform along its length of the two. Then the extreme flexibility of very high E.M.F. carbons was an objectionable feature. It was difficult to give any definite figures as to the relative life-values of high and low E.M.F. lamps, because incandescent lamps in practical work were usually run in a most haphazard sort of way as regards temperature, thus quite stultifying the value of the results obtained in respect of duration. With regard to central station lighting work in America, he could corroborate everything that Prof. Forbes had said, as he travelled through most of the Eastern States last autumn. One was astonished at the energy and activity of the Americans throughout the States, small and large, and at the amount of work being done. The causes were so very various that they would require a great deal of discussion to get to the bottom of them. The Americans had one advantage which electricians in this country could not obtain by any means in their power; they were favourably handicapped in the first place by the fact that the gas interests there were very small, and, further, by the fact that, generally speaking, the gas was not so good as in this country. Gas was not really an established going concern in America in the sense it was here. Here the public had gas and had had it for many years, giving fairly satisfactory light, and they did not trouble themselves much about any other illuminant. In America it was not so, and Americans having the two alternatives to choose from, *ab initio* recognised the value of electric light and employed it.

With reference to Mr. Edmund's paper, he asked what provision Mr. Edmunds made in his system to cut off the charging current when the cells were completely charged. At one time it was thought with regard to secondary batteries that you could go on charging and charging, without leading to any serious results, and that only under-charging or too rapid discharging destroyed the batteries. But he believed that recent experience showed that over-charging was equally damaging with under-charging. When cells were fully charged they commenced to bubble, decomposition of the water took place, and the gases gave off at the surfaces of the two plates. The rising gases would thus gradually detach the peroxide material upon the surface of the plates. He thought those who had secondary batteries would find that that was the case where they had over-charged the plates; they would find the peroxide thrown to the bottom of the cells in a powder. It appeared to him to present a point of some difficulty in Mr. Edmunds' system. He would further ask Mr. Edmunds what was the amount of energy lost in his automatic apparatus. He understood that Mr. Edmunds proposed to put one of those automatic apparatus with a set of cells in each house.

With regard to Mr. Lowrie's cell, he (the speaker) understood from the President's explanation that the primary cell was placed in the discharging secondary circuit of the lamps.

The President: So I understand.

Mr. Sellon said surely it would be a matter of some difficulty to introduce in every case a primary cell to take the whole of the discharging circuit, and he asked what was the loss in that cell itself.

Mr. Sydney F. Walker said with regard to the 100-volt and 50-volt lamps, his experience was the same as that of Sir William Thomson. A 100-volt was quite as good as a 50-volt lamp, and so far as he could understand, it was a question of manufacture; that was to say, the greater perfection of the low-volt lamps at first was due to the fact that a greater number were made. In the early days, only the 50-volt lamps were reliable; when the 60 and 80-volt lamps first came out they were unreliable. Filaments were very delicate, and even the jarring of a railway carriage, or the shaking of a building often parted them; but with the advance of manufacture those difficulties seemed to have been got over, and, so far as he knew, high volt lamps now were every bit as good as those of low voltage. He was quite sure that any saving that might be made by using 50-volt lamps would be more than counterbalanced by the extra cost of wiring; the interest on the money representing the extra outlay would much more than pay the difference. They would do far better to save their copper and use 100-volt lamps. He would go even further and say use 200-volt lamps, and save still further in that way. With regard to the system which Prof. Forbes had described he did not think there could be two opinions. They must all admire the energy and skill with which the whole thing had been worked out from end to end. It showed what a highly educated man, with plenty of capital at his back, could do. He wished they had many men of Mr. Westinghouse's type in this country; public electric lighting and other kinds of electrical work would then go ahead far faster than they had hitherto done. He would like to ask Prof. Forbes if he could tell them what material was used to insulate the coils in the transformers from each other. It appeared to him that the weak point of the transformer was in the insulation of the primary from the secondary. He was pleased to see that Mr. Westinghouse only used 1,000 volts instead of 2,000 volts in his mains. This gave an increased margin for safety. He thought he was borne out by some recent experiences in London in saying that those high voltages would possibly lead to trouble; they had, he believed, led to trouble already. With regard to distribution and divided mains, he was also pleased to see that Mr. Westinghouse had worked out in a practical way a method of dividing up completely. It appeared to him to be by far the best plan for every dynamo and every engine, as far as possible, to be connected with its circuit and stand its own work and own risks. With regard to the question of accumulators *versus* transformers, he should certainly prefer accumulators, but should be rather chary about placing an accumulator in a house by itself. The weak point of Mr. Edmunds's system was the possibility of leakage between the charging main and house service. He would be glad to know what provision was made to prevent leakage. Another weak point was in the automatic apparatus. Automatic apparatus occasionally got out of order. He should certainly prefer, in a case of that kind, to have local stations under the charge of a man, in the same way as he believed Mr. Crompton did in London.

Major-General **WEBBER** said he thought Sir William Thomson remarked, on the subject of distribution by means of accumulators, that any invention which would promote a system in connection with them, and at the same time reduce the large cost of using them would be received with great favour by all scientific men. They must all feel indebted to Mr. Edmunds for the admirable description of his distribution system he had given, and those who had known Mr. Edmunds as long as he had would be sure that his work was the outcome of a great deal of pains and thought. He (General Webber) was to some degree acquainted with his views, and he believed he was right in saying that in the last few months he had taken out some fifteen or twenty patents in connection with this object. But the Association must not run away with the idea that distribution by accumulators in the old time—that was, six or seven years ago, as attempted by Mr. Brush in America, and by what was known as the B.T.K. combination at Colchester—represented all that had been successfully done in that direction; indeed, it was certain that in both those cases there was not success, and that since then until Mr. Edmunds came before them that day no attempt had been made to use accumulators. On the contrary, it was only necessary to look at Mr. Crompton's work at Kensington to see that the subject had been well worked out in at least one instance. They must reduce the value of Mr. Edmunds's invention to the extreme ingenuity with which he had designed his distributor; the rest of his system might be fairly said to have occurred to many, who had only been deterred from proceeding on the same lines by the great cost of accumulators and by the natural objection of experienced engineers, who dealt with electricity to automatic apparatus intended to work without supervision. When he first saw the system he was strongly reminded of the Schafer multiple telegraph, the most complicated instrument ever shown to any public. At the same time, he would be the last to say that, because Schafer's system did not come into use for telegraphing, that of Mr. Edmunds would not come into use for electric lighting. It must not be forgotten, however, that those who were looking forward to accumulator stations, where accumulators would be concentrated, did not believe in automatic instruments, especially if a number of them was to be used in the system. They proposed to place two or three thousand accumulators at one station and distribute hence to the surrounding neighbourhood. That was not Mr. Edmunds's object, and he could see very little *raison d'être* for the latter's system. Mr. Edmunds was going to put a milch cow into everybody's house. Whether that was a convenience or otherwise had yet to be proved. Not long ago the Institution of Civil Engineers told a friend of his (the speaker's), who wanted a very beautiful engineering apparatus to be brought before the Institution, that they never allowed anything of that kind to be shown until it had been in practical working or had become a commercial success. He thought, as an admirer of Mr. Edmunds and a strong advocate of the objects he had in view, that it would have been better in all respects, for electricity and himself, if he had waited 12 months and then told him that his system had been working and had done its duty. Then, and only then, could they regard his invention as coming within the range of practical use.

Mr. **KAPP** said the question of the best potential to use was a very important one, and one which ought to be settled, if possible, by an association such as that or the Society of Telegraph-Engineers and Electricians, so that a contractor who had to put up a central station might have a choice of apparatus from various makers. It might be very important to be able to order transformers by telegraph without having to specify period or potential and yet be sure of getting the right thing; but the tendency in this country was, with plants of moderate size, to go for 2,000 volts. Mr. Ferranti, he believed, went as far as 10,000 volts, but his station stood by itself. He would ask Prof. Forbes if there was any reason whey they should go to 1,000 volts. It might be that they were all wrong in accepting 2,000 volts, but unless there was some clear evidence on the point, the introduction of the 1,000 volts potential would result in a double standard, which was very undesirable from a commercial point of view. Therefore, unless Prof. Forbes could show some substantial reason for the adoption of 1,000 volts, he thought they should stick to the 2,000 standard. Prof. Forbes said that under the Westinghouse system, although they could couple two machines in parallel, they did not like to do so. Now, the current must be on night and day, every minute and every second, and he would like to ask, if they did not couple two machines in parallel, how could they change the machines from one circuit to another, and how could they prevent the interruption of the light for a few seconds? And that few seconds in a theatre would be sufficient to create a panic. Therefore he thought their endeavours should be directed to find a way of coupling in parallel. Dr. Hopkinson had shown theoretical reasons why it should be done; Prof. Adams had done it at South Foreland, and Mr. Westinghouse in America; Zipernowski was doing it in all his installations, and if they could do it surely others could also. Did the transformers get hot, so hot that the hand could not be borne upon them? That was a great failing with transformers he found. Recently he had a conductor of 260 mils. on the circuit and this got hot, he could not make out why. The iron kept cool and the copper got hot, just the opposite to what usually occurred. He put in a continuous current and the rise in temperature was 30° C. less than with the alternating current. He could see now why that was so. Sir William Thomson told them that they lost the advantage of thicker copper beyond a certain point. The question of period was also an important one, but he did not think it was wise to go too fast. He began with a period of 100 complete cycles a second, which was 12,000 complete cycles a minute, and he had gone down by steps until he had arrived at 9,000 alternations; no great amount of iron or copper was wanted, but they had the advantage in the dynamo. The Westinghouse dynamo was much heavier than any English dynamo of the same output. The output of the Westinghouse in comparison with the weight did not seem very great. There were various others which gave an output of two lamps for every 2 lbs. of dead weight. Mr. Westinghouse's, he believed, gave two lamps for every 4 lbs. Speaking generally, he thought the Americans used the instruments and the English only made them.

Prof. **FORBES** said, before he passed on to answer the questions asked upon his own communication, he would like to make just one remark about Mr. Lowrie's paper. When Mr. Lowrie spoke of meters, he said that hitherto the waste of energy had always been too great, and that alternating current meters in the past had not been successful. He could not let that statement go unchallenged. There was an alternating current meter of his which did not use too much energy, and which had worked extremely well. They were beginning to make this meter in considerable numbers, and he had just arranged with Mr. Groves to proceed with the manufacture for an indefinite time. The resistance of the meter, which he showed before the Association last year, was $\frac{1}{100}$th of an ohm; it began to read at 1 ampère and was capable of reading as far as they liked to go with it. For 30 lamps it used nearly half a volt. Coming to the questions put to him, Sir Wm. Thomson remarked that it was a question between alternating currents and constant currents. There were many cases, undoubtedly, in which a continuous current would be best, and because he had been speaking of the alternating current as used in America it must not be therefore supposed, as Mr. Kapp seemed to think, that he was advocating it. He wished not to advocate then anything that the Americans did, but merely to lay before them what was being done in America; he did not wish to be looked on as saying that everything done there was right. He thought there were a great many things which they could do better themselves. He quite agreed with what Sir William Thomson had said about accumulators. He had always felt that accumulators would be very desirable if they had the qualities which Sir William mentioned. Accumulators were now working extremely well. They had fulfilled the conditions, so far as that was concerned, already, but unfortunately they were still far too costly, a matter which he hoped would soon be remedied. Sir William asked him about the working in parallel of the alternate current machines, and Mr. Kapp also made some remarks upon the same subject. Of course it was known by Mr. Wylde that they could be run in parallel, and there had been various little experiments that they knew of in this country showing that alternate current dynamos could be run in parallel. He knew that in the old days of the Grosvenor Gallery it was tried with Siemens machines, and he knew that it failed. Driving from the same engine, but with different belts to two dynamos, they could not get them to work in parallel. But the Westinghouse dynamo could be worked in parallel, as also could several others which he had seen; he had seen Thomson-Houston alternating current machines running in parallel, and he fancied that Dr. Hopkinson showed in his paper before the Civil Engineers several years ago that alternating current machines could be run in parallel, but not in series. The reason why they checked each others working in parallel, was that the lag in phase helped to check the one that was going too fast and to check the force which was pulling against the other one which was going too slow, thus making it go faster. The amount of this force depended on the amount of the self-induction of the dynamo machine. The Westinghouse machine had far more self-induction than the Siemens, and he fancied that was the reason the American machines worked successfully in parallel, whilst the Siemens did not. He wished to make perfectly clear what he was thinking about. What he meant was that at present all alternate current machines, driven each by a belt, each by a separate engine, when they are once started in unison by the arrangement which he described would keep in unison by slipping of the belt. It was slipping of the belt which allowed them to act as checks to each other even though the engine did not vary. At the same time the checking effect was much greater when the maximum current was passing through. It was only when they were doing a good deal of work that the synchronism was perfect. If they were much under half load they got out of synchronism, and one got half a period ahead of the other sometimes. If one only were under load they would not be in perfect synchronism: that was shown by the flickering of the lamps: the E.M.Fs. of the two machines would then be nearly opposing each other. Mr. Kapp's question as to how they were to change dynamos if they could not run in parallel was of course answered by what he had said in reply to Sir William Thomson. There was no difficulty about running in parallel.

Mr. **KAPP**: There must be a period when the lamps are out.

Prof. **FORBES**: Oh, no. You may first run on the one, then put on the other, and then take either off.

Mr. **KAPP**: Then there is a period of running in parallel.

Prof. **FORBES**: Oh, yes; there is no difficulty about that. The Alleghany River separates Pittsburg from the town on the other side, and when one centre is hard up for electric current it is often helped by power being switched on from the other side of the river.

Sir **WILLIAM THOMSON**: Then inversely a current will synchronise with another at a great distance?

Prof. Forbes: Yes. Proceeding, he said then came the question about 100-volt or 50-volt lamps. He would not express any opinion about that, seeing that several people had already expressed themselves differently; he would leave that to be settled elsewhere. Certainly in America it was believed that the durability of the lamp was very much greater with the smaller potential. Indeed, he thought that the very fact that, though before they had alternating current machines they urged makers to go to as high tension as they could, they had never got higher than 120 volts was an indication that the greatest economy was not to be found in high voltage.

Sir **William Thomson**: There is a difficulty in making a high volt lamp at all in England.

Prof. Forbes said it was a very difficult question, and as he was there only to tell them what was being done in America he did not wish to take part in a discussion upon that subject just then. With regard to the number of alternations, what Sir William Thomson had told them was extremely interesting. When he first used his meter with an alternating current he expected to find the resistance increase, but it did not; but that was with a much thinner conductor. As to what was meant by the statement made to him about the hydrogen natural gas weakening the steel he did not like to say, but he imagined that there was some occlusion of hydrogen gas in the iron boilers which did produce some effect. He really could not do more than simply make the statement that was made to him. They found that there was a little deterioration in the iron.

Lord **Rayleigh**: I think it has been observed that electrolytic hydrogen will much deteriorate the iron when it is deposited.

Sir **William Thomson**: When the iron is at high temperatures.

Prof. Forbes, continuing, said Mr. Walker asked about the insulation. The coach varnish of which he had spoken was used. The insulation was extremely thick, and he was perfectly sure it was impossible to have connection between the primary and secondary. With regard to the question of 1,000 or 2,000 volts potential, his own notion was that probably they would work with 1,000 volts for small stations.

Sir **William Thomson** said if they could get over the practical difficulties in the way of using 2,000 volts, they would only need half the quantity of copper. Taking the proportion of reduction at 40 to 1, if they had 50-volt lamps, they would get twice the benefit from 2,000 volts tension, and ten times the benefit with 10,000 volts.

Prof. Forbes said it must be remembered that after a certain stage, if they went to a high enough potential, then copper was a much smaller item than when they were dealing with a low potential, when copper was chiefly to be thought of. But it occurred to him to suggest that it might be a possible thing in future that they might have a number of small stations, say, up to 1,000 volts, and going to short distances, when there would be no difficulty about the copper mains, and that eventually they would be combined into one large scheme, supplied from 10,000 volts. As to the heating of transformers, 50° C. was the greatest rise allowed in temperature, and the hand could always be borne upon them. With regard to the number of alternations, it was desirable that opinions should be expressed, and he hoped electricians would soon begin to talk about the matter, and come to some conclusion as to what the alternations should be. It was not fair to manufacturers that electricians should be undecided on that point, for it put a difficulty in the way of their constructing dynamos suitable to the requirements.

Mr. Edmunds, replying to the criticisms upon his paper, said Sir William Thomson pointed out the advantages of accumulators as compared with transformers, and he had some figures there, which were only approximate, but, he thought, rather interesting, as bearing on the actual cost of an installation of certain dimensions. The estimated cost of plant for 10,000 10-C.P. lamps would be about £25,000, and at 6d. per unit the light would be equivalent to gas at 3s. 11d. per thousand feet, and that would give a probable profit on the capital of 21 per cent. If they went to a larger plant of 15,000 lamps they would find the cost to be about £37,000, and that would be equal to gas at about 3s. 6d. per thousand feet. A plant of 20,000 lamps would give light at a cost equal to gas at 3s. 5d,, and 40,000 lights would be equal to gas at 3s. per thousand feet. With regard to voltage, from the experience he had had he should say the lesser voltage would be more generally suitable than the higher one, for several reasons. With the lesser voltage they could have lamps of smaller C,P. than otherwise. In some cases it was very useful to have a one, two, three, or four C.P. lamp, and the lower the voltage the better could these small C.P. lamps be made. With regard to their longevity, he did not think there was much in it. Mr. Sellon asked a question with regard to fig. 2, and wanted to know whether the battery A was in connection with the discharging circuit and the charging circuit. Never. Mr. Sellon also asked what provision was made for removing the cells when fully charged. A voltage regulator was employed for that; it was now being worked out and protected, and promised very well. It appeared that when the E.M.F. of the cells was raised to a certain extent, say to 50 volts, gradually, a higher E.M.F. was gradually attained as the charge continued, and when a higher voltage was reached the cells automatically cut themselves out of the charging circuit, but they cut themselves in again, not when the potential was reduced, but when the discharge on the local circuit had arrived at a given amount. The energy lost in the apparatus was not large, because although the intervals might be every two minutes, the magnets were only in operation for three-quarters of a second. With regard to the remarks of Mr. Walker and General Webber, he must say that he felt that with any kind of apparatus there was the danger that it might sometimes stop or do something that it ought not to do, and he had found a great many difficulties in that respect; but he was glad to say now that after some four months' trial in their own experimenting room, they had had an apparatus at work without attention for some three or four weeks in the Cadogan circuit. Of course, in a system of such complication there were many points still requiring attention; but he did not think there was anything that had been overlooked which would interfere with the practical operation of the apparatus. What he claimed for the system was that it could supply a very large amount of work, giving a constant supply to each house or each centre with only one-third of the apparatus that would be required for most other direct systems, including the same number of cells in series in the charging main. They did not, however, confine themselves to giving those charges to each house in a district, but so far as possible grouped them so as to supply several houses from one or more centres.

Mr. Lowrie (who was prevented from being present when his paper was read, but came in during the progress of the discussion) said that about 50 or 60 of his meters were now in use, and one had been working for about eight months. They were checked and tested very carefully. One gentleman, a consumer, kept a very careful check against the company by noting the number of hours during which the current was used, and he said that the amount they had charged him with was within half an hour of his own computation.

Lord **Rayleigh** said he saw that the general idea of Mr. Lowrie's method was to superimpose the constant E.M.F. on the alternating current, so that the deposit in the electrolytic cell would be a measure of the time during which the lighting was in progress. One of the arrangements, fig. 3, showed that the constant E.M.F. was placed in the primary circuit of the electro-depositing cell in the secondary. He could not see how a battery in the secondary could have any effect at all.

Mr. Lowrie: By induction it does. We have other batteries which are not shown there.

The **President**: I presume, Mr. Lowrie, that the plan you are using graduates the two?

Mr. Lowrie: Yes. When the auxiliary E.M.F. is in the primary there is a very pronounced error.

Lord **Rayleigh** said he perfectly understood that, but still could not see how the arrangement as shown could have any effect.

The **President** asked what provision was made to secure that the current to the secondary battery introduced was kept constant? The efficiency of the meter depended on the constancy of that current.

Mr. Lowrie said they went round once a week with a delicate voltmeter and tested the meter, and if there was anything wrong they put in another. Then it was left to itself for the rest of the week.

The **President**, after conveying the thanks of the Section to the three gentlemen whose papers had just been discussed, said there was only one remark that he would like to make in addition to what had been brought forward, and that was to protest against the assertions that were made that the whole of the inaction in England was due to the Act of 1882. That Act had in no sense interfered with the action of Corporations.. There were 60 or 70 Corporations in this country who took provisional orders under that Act and never carried out the provisions of those orders. The fact was that the system of electric lighting had not then, and had not even now, sufficiently advanced to justify corporations in investing the money of the ratepayers in electric lighting enterprises. Even there that day they had had advocates of rival systems. He did not think there was a single independent electrical engineer who could conscientiously say that any one of those systems was best; and that was very much the reason why they had not gone ahead. And he was very glad they had not gone ahead. Prof. Forbes had just come back from America. He (the President) went to America four years ago and came back with a species of Yankee-mania. It was impossible not to be impressed by the go-aheadedness of Americans; but when they came back and settled down again they could see that in this country they were, after all, progressing more surely, if more slowly. With regard to the relative advantages of 100-volt and 50-volt lamps, it was a question of great importance, and he should make a strong recommendation to the Institution of Electrical Engineers to form a committee to take into consideration the relative advantages of different voltages. At present he was strongly in favour of 100-volt lamps. He did not think the evidence in favour of 50-volt lamps could be accepted. They had other things besides secondary generators to think about. They had the parallel system, the system of accumulators, and, more important than all those, isolated houses where dynamos and batteries were used. He had, after five years' experience, come to the conclusion that for general purposes 100-volt lamps were by far the best. The batteries lasted twice as long, and altogether the higher the E.M.F., for battery purposes, the better and more economical. Having tried all voltages, he found that wherever he had used 100-volts there were advantages not obtained with the 50-volts. He would like, had there been time, to have said something about the extreme ingenuity and ability with which Mr. Edmunds's arrangement had been carried out. It certainly had elements of success, and when rubbed down by experience would doubtless prove an economical mode of distributing electricity and compare favourably with other systems.

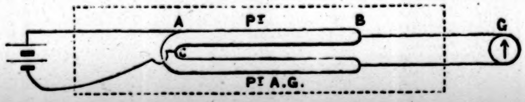
ON AN APPARATUS FOR DETERMINING TEMPERATURE BY THE VARIATION OF ELECTRICAL RESISTANCE.

By WM. SHAW, M.A.

(Section A.—Mathematical and Physical Science.)

Monday, September 10th.

THIS apparatus is designed to measure to a high degree of accuracy the mean temperature of a large burette immersed in a water bath. A Wheatstone quadrilateral is formed by joining the ends of a platinum wire about 30 inches long (resistance about 11 ohms) to those of a platinum silver wire, B, C, D, A. The platinum wire is bare, and the alloy is silk-covered ·007 inch in diameter. Its resistance is 13·5 ohms per metre. At A, C and B, D respectively, battery and galvanometer wires are soldered. The lengths of the wires are so arranged that for some temperature near 15° C. there is a perfect balance indicated in the bridge.



The wires are laid in parallel lengths, and disposed upon a strip of pure India-rubber about ¾ inch wide in the manner indicated in the fig. The India-rubber is painted with a solution of India-rubber in benzene, and a second strip, similarly painted, is laid down upon it, and the wires are thus completely enclosed; the strip continues 18 metres at each end beyond the junctions of leading wires, so that the whole length of strip is about 5¼ feet, and from the two ends the galvanometer and battery leads respectively project.

A very flexible quadrilateral is thus obtained in the form of a ribbon, which can be wrapped round any apparatus whose temperature is required. Since the two wires, which together form the quadrilateral, have different temperature coefficients, there is no balance except for one particular temperature, but the balance can be restored by shunting either the wire, A, B, or the arm, B, C, by connecting a resistance box in a suitable manner with the projecting leads. The resistance of the shunt required to produce a balance can be employed to indicate the temperature when the instrument has once been graduated. The graduation of the apparatus exhibited was made for the author at the Cavendish Laboratory by Mr. J. W. Capstick, demonstrator at University College, Dundee. The ribbon was wound round a metal cylinder, and the whole immersed in ice or water, and a number of readings of the shunt taken at different temperatures between 0° C. and 28° C. The conductivity of the shunt at different temperatures is very nearly represented by a straight line cutting the zero line at the neutral point, 15·16° C. The maximum divergence from the straight line corresponds to the temperature difference of a fifth of a degree between 0° and 15·16°, and to about a tenth of a degree between 15·16° and 28°. In the graduation the temperatures are referred to the Kew standard by means of mercury thermometers previously compared at Kew. The temperature can be read with a suitable galvanometer to about the 500th of a degree.

ON THE C.G.S. AND PRACTICAL UNITS OF MEASUREMENTS.

By W. H. PREECE, F.R.S.

(Section A.—Mathematical and Physical Science.)

Tuesday, September 11th, 1888.

IN the C.G.S. system, which may now be considered to be in universal use, the quantity of heat which will raise one gramme of pure water one degree centigrade from the standard temperature, 4° C., is the C.G.S. thermal unit, and is frequently called the *calorie*, but the term calorie is just as frequently applied to the kilogramme water degree. Its mechanical equivalent is 42,000,000 ergs. Since heat is a generic term applied to that form of work which is expended on any substance when its temperature is raised, it seems desirable that the thermal unit and the unit of work should be more closely allied.

The watt has been universally accepted as the unit of power; it is the unit rate of doing work, it is 10^7 ergs done per second. Now, Sir William Siemens, at Southampton, 1884, proposed to call 10^7 ergs a joule. Hence a watt, or the work done by a coulomb, or by an ampère flowing for one second, is a *joule per second*, and there would be 4·2 joules in one calorie. Thus, ·2381 gramme of water raised one degree, or one gramme of water raised ·2381° would equal one joule. We cannot, unfortunately, avoid the use of a coefficient. The arbitrary character of the degrees of temperature prevent this, but the relations between the quantities are very simple.

One joule	= 10^7 ergs.	= ·24 calorie
One watt	= 1 joule per sec.	= ·24 calorie per sec.
One calorie	= 4·2 joules	= 42,000,000 ergs.
One British thermal unit (pound water degree Fahr.)	= 1,058 joules	= 252 calories.

Let us take a glow lamp having the following elements:—

	C.G.S. Units.	Practical Units.
E.M.F.	= 100×10^8	100 volts.
R	= 200×10^9	200 ohms.
C	= $\cdot 5 \times 10^1$	·5 ampères.
E C	= 50×10^7	50 watts.
Candle power:		20 candles.

$$H = \frac{C^2 R}{J} = \frac{\cdot 0025 \times 200 \times 10^9}{4\cdot 2 \times 107} = 12 \text{ calories.}$$

Thus, 50 watts develop 12 calories per sec.
4·2 watts develop 1 calorie „ „
1 calorie per sec. develops 1·7 candles.

It is quite clear that the acceptance of the joule will facilitate many calculations and prevent much confusion. While on this question it is well to point out the extreme inconvenience that is rapidly increasing, owing to the incessant necessity for using C.G.S. units in magnetic questions, in having the ampère and the C.G.S. unit current different. This is the consequence of having accepted the volt as equal to 10^8 instead of 10^9 C.G.S. units. The result is that the ampère is $\frac{1}{10}$th of the C.G.S. unit current. The only reason ever given in favour of the accepted volt is that it is very nearly the E.M.F. of a Daniell cell. This was a good reason 25 years ago, when telegraphy was the principal practical application of electricity; but now, when we are rapidly extending the application of electricity in all directions, when we are using higher multiples of volts every year, the time has come when we should seriously consider the advisability of making the volt 10^9 instead of 10^8 *C.G.S. units of E.M.F.*, and so make the ampère and the C.G.S. unit of current the same.

Again, it is perfectly clear that 106 centimetres of mercury at 0° C., 1 square millimetre in section, is only a very rough approximation to the true ohm, and that 106·3 would be a much closer approximation, and be very near the truth. The roughness of the approximation of the legal ohm has prevented its general acceptance, and there will be no universal acceptance of one and the same unit of resistance until we agree on the value of the true ohm. The legal ohm was accepted in 1883 provisionally for 10 years. There will be a congress of electricians in Paris next year, and if we English electricians proceed there armed with the opinions of the British Association Committee, that 106·3 can, in their opinion, more nearly represent the true ohm, and that the volt should be made equal to 10^9 deg. C.G.S. units of E.M.F., it would have very great weight in the final determination. It is most desirable also that we establish and name unit magnetic field, but whether this is to be based on the C.G.S. units or on the practical units must remain open for consideration and discussion.

There is one other point to which I wish to direct the attention of the committee. It is the value of the standard of pure copper. We have hitherto accepted implicitly Matthiessen's standard. According to his experiments the specific resistance of a cubic centimetre of pure copper at 0° C. is 1,642 C.G.S. units, but this is known to be about 3 per cent. wrong, and it is most desirable that experiments should be made to determine the proper figure. The resources of the Cavendish Laboratory, where so much good work has been done, might be devoted to this desirable end. I recommend:—

1. That the practical unit work, 10^7 ergs, be called the joule.
2. That experiments be made to determine the specific resistance of pure copper.

N.B.—The committee choose the term *therm* instead of *calorie* for the thermal unit.

THE COMPARATIVE DANGER OF ALTERNATING v. DIRECT CURRENTS.*

By Dr. P. H. VANDER WEYDE.

IT is only a few days ago that I received the flattering invitation to appear before this meeting of the most eminent electricians of the United States, and present a paper containing my conclusions about the comparative danger to life lurking in the alternate and direct dynamo currents. I do so readily, as I have lately had the opportunity to become acquainted with the arguments made on both sides of the question. I was not prepared to do so when, a few weeks ago, I attended a meeting of the New York Board of Electrical Control, where several papers were read, most of them in opposition to Mr. Harold P. Brown, who had expressed himself in opposition to the use of the alternate current system, the introduction of which has lately been commenced. When the president of that meeting asked me to express my opinion I was compelled to decline, stating that my appearance there was solely as a listener to arguments upon the subject, about which I desired full information.

As some of the papers read there were written in such a spirit as to compel Mr. Brown to defend himself, I made up my mind to attend his expected defence, especially as it might give me an opportunity to hear what powerful arguments and facts would be brought out by the person who has put himself forward as the champion of the enemies of the alternate current system.

It always makes a painful impression upon me when I see that a scientific investigator feels the necessity of defending himself against personal attacks. The calling of the scientist should simply be confined to ascertain what is the truth. If he thinks he has detected erroneous conclusions arrived at by any co-labourer it is his duty to expose them. At the same time he must mind the old rule, *Suaviter in modo, fortiter in re*; therefore, above all, he must let the facts speak for themselves, avoiding any allusion regarding the capabilities or ulterior motives of his antagonist. In short, he must leave the use of all personalities to the politicians; for these the main considerations are *persons*; *principles* are secondary, and may be changed to suit circumstances, or thrown overboard entirely. In science, however, we have, on the contrary, nothing to do with *persons*, but only with the ultimate *truth*, which we are all striving to arrive at.

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* Paper read before the National Electric Light Convention of America.

At a recent visit to the physical lecture room of Columbia College, where I had been invited by telegram to hear the reading of a paper and a lecture illustrated by experiments by Mr. Harold P. Brown, the feeling referred to above was more painful to me than even the howling of the dog which was experimented upon. Dogs have for so many years been considered as the legitimate martyrs of science, especially under the hands of physicians and professors of physiology, that we have become more or less hardened in their regard. Nobody will deny the immense value of the increase of our biological and especially pathological knowledge obtained in this way.

A great portion of the paper read by Mr. Brown consisted in complaints about the personal attacks made upon his capability, as soon as he asserted in print that the alternate electric currents were more dangerous to life than the direct currents, namely, that a current of the electromotive force of, say, 300 volts alternate current, would be more likely to destroy life than double or triple that strength of a direct current. One feature was prominent in the reading of the paper as well as in the lecture and experiments. It was evident that the purpose was not so much to exhibit what was the truth in the matter as to show that he was right in what he had asserted. He attempted to prove this by experimenting upon a large dog, which, when subjected to a direct current, stated to be 300 volts, set up a short howl; 600 volts produced a longer howl and frantic efforts to escape from the cage, while a current of some 900 volts paralysed the dog, who kept up a laboured breathing. Then a representative of the New York Society for the Prevention of Cruelty to Animals interfered and forbade further trials of this kind, so that Mr. Brown was compelled to give the dog the *coup de grace* by applying the alternate current, which he said was of the strength of only 300 volts.

Unfortunately for Mr. Brown, this proved nothing, as the dog was already half dead by the previous successive and increasing direct current doses of 300, 600 and 900 volts respectively. Another dose of 300 or 600 volts direct current might in all probability have ended his life just as well. But what is more, his statement that the alternating current was merely 300 volts was not proved, as he himself had stated previously that reliable instruments for measuring alternate currents were difficult to obtain, and confessed that he had only a voltmeter for this purpose but no ammeter. After the lecture I examined the voltmeter and found, that according to the statements of Mr. Brown himself its operation was based upon indications of rise in temperature.

Now, it is well-known that voltmeters based on this principle are based on false premises. Rise of temperature is not produced by electromotive force, but by amount of current: one Wollaston plunge battery cell, of one square metre acting surface, having, of course, the electromotive force of only one single volt, will produce far more heat than a series of 24 freshly prepared cups of a Leclanché battery of 40 volts electromotive force. This pretended voltmeter was, in fact, a kind of ammeter.

This is so self-evident that Prof. G. Forbes, of England, who last year exhibited before the American Institute of Electrical Engineers in New York a meter for alternate currents, did not think of calling it a voltmeter, because its operation was also based upon rise of temperature, but he called it a current meter. This meter was described and illustrated in all the electrical journals at the time, where there was added a little table giving the observed currents in ampères. Immediately under that table follows the statement that Sir William Thomson complimented the author on having practically solved a problem on which he himself had been working without satisfactory results, thus endorsing the instrument as a current or ampèremeter. This shows that Mr. Brown was mistaken when he said he had no ammeter for alternating currents; he had one and supposed it to be a voltmeter. If he used it as such, what becomes of his statement that the alternate current he used was 300 volts?

And here I cannot let the opportunity pass by of repeating what I have been preaching for years—namely, that the voltage alone is no criterion in regard to the danger of a current; the danger comes only when the electromotive force (as measured in volts) is accompanied by a sufficient quantity or volume of current (as measured in ampères). An electromotive force carrying a current of only a few milliampères may be as high as 1,000 volts without doing any harm (even when alternating, as proved by the Ruhmkorff coil). It behaves like water oozing out of a pinhole leak under pressure of hundreds of pounds; it can do little or no work. On the other hand, a very large volume or quantity of current with small electromotive force, of say a small fraction of a volt, will not have the power to pass through the human skin, or even through the liquids of the body, but only through metals (as proved by the thermo-electric pile). Such currents behave like a large volume of water in a lake, with little or no inducement to make it move; hence this will do little or no work.

Prof. Mayer, of the Stevens Institute, Hoboken, in a lecture recently given to the members of the New York Electrical Society, the Electrical Section of the American Institute, exhibited some novel experiments, intended to measure, if possible, the voltage of static currents. He used an old-fashioned friction electric machine, and showed that the voltage of its currents ran into the thousands, while the ampères were so minute as to escape the possibility of measurement with the means thus far at our disposal.

This leads me back to the subject of the experiments of Mr. Brown; he stated that the quantity of the direct current he used was only a fraction of one ampère. This induced me to examine also the ammeter after the lecture, when I found that the ammeter and voltmeter were coupled together in series in the same single circuit. Now, every experienced electrician knows that in order to measure the capacity of a dynamo current in ampères we must not place any other resistance in the circuit, much less the high resistance of a voltmeter, which may be 500 ohms, as then the indication of the ammeter may drop down 90 per cent. of the original indication; in fact, the ammeter indicated actually two-thirds of an ampère, as had been stated by Mr. Brown when he was asked during the lecture.

The manner in which the ammeter and voltmeter were used was considered by me in connection with the fact that, if really a current of only two-thirds of an ampère had been obtained, it would render the dynamical value of the three currents used only equal to 200, 400, and 600 watts respectively, which was incompatible with their effect on the dog. This caused me to go to the dynamo plant of Columbia College to enquire. I found that the direct current machine used was an Edison municipal dynamo of 15 ampères and 1,200 volts.

It is likely that Mr. Edison gave it the name of municipal in anticipation of the fact that the municipal governments will need such machines when the new law in regard to capital punishment and of despatching criminals by electricity instead of hanging goes into operation next year. If so, it is very suggestive, in regard to the controversy we are discussing, that such an able electrician as Mr. Edison is satisfied that a direct-current dynamo is all that is needed for that purpose, without regard to the alleged greater effectiveness in this respect of the alternate current dynamos. This dynamo did not belong to the Columbia College plant, but it was furnished by Mr. Edison for the occasion, as well as an alternate current dynamo. Both were removed after Mr. Brown finished his experiments, which, it appears, he continued privately for a few days after the lecture.

One of the dynamos used belonged to the college plant, it was an incandescent light dynamo, of which the capacity was stated by the engineer to be 125 ampères and 110 volts. This dynamo was employed to obtain the alternate currents, by using it to feed the field-magnets of a Siemens-Halske alternate current machine, after the usual manner, as represented in the engraving (exhibited), which shows the essentials of the arrangement used, only with this difference, that the field magnets of the Siemens alternating machine were supplied by the direct currents obtained from the much larger 125-ampère dynamo referred to, in place of the small one represented in the engraving.

The kind of apparatus which was used and the nature of their arrangement for the production of the alternate current makes it very unlikely that it would produce a current of only 300 volts, especially when we take into consideration that the current developed must represent some kind of equivalent for the horse-power required; firstly, to drive the 125-ampère incandescent light dynamo; next, that required to drive the alternate current dynamo. The latter must have considerable power, as the 125-ampère current was more than sufficient to saturate the great number of its stationary electro-magnets. According to the statement of the engineer, the engine driving the 125-ampère dynamo requires 36 H.P., that of the alternate dynamo 36 H.P.; together, 72 H.P., which, at 746 watts per H.P., gives 54,712 watts. If the ampères of such a current had been measured after the method employed by Mr. Brown for the direct current, it would have resulted in 54,712, divided by two-thirds of an ampère, or 82,063 volts.

Of course, this is an exaggerated estimate, but sufficient to show the worthlessness of all the figures as given by Mr. Brown. In order to come to correct conclusions it would be necessary to measure, by means of indicator diagrams, the engine-power utilised, and measure the currents obtained by proper instruments, properly used and conscientiously observed. One thing is certain, namely, that the dynamos used were not of such a defective quality as to warrant the supposition of such an enormous loss of the power applied, as would be the case if only an electromotive force of 300 volts had been obtained.

I must also not omit here to state that if Mr. Brown had known the proper quarters where to apply for apparatus to determine properly the voltage and ampères of alternate currents, he could have been provided, and his labours in this direction might have had some value.

There was added on the lecture room table another important additional (however small) contrivance for the right understanding and use of which I will have to make a short introductory explanation. I do this, also, because I have been especially requested to do so by several parties who had become interested in the operation of this, to them, very novel device.

I must ask pardon to bring some elementary considerations before this learned body, but my apology is that this paper will be published and read by many who are not so well informed about electrical subjects, and whose ideas upon it need some clearing up.

Everyone who has handled faucets attached to pipes from which water can escape under pressure, will have noticed that when suddenly shutting off the water, a blow like that of a hammer is produced in the pipes. This is caused by the momentum of the water when in motion (often very improperly called inertia), and which shows itself every time this motion is arrested. On this peculiarity is based a contrivance in which, by the automatic alternate opening and closing valve, a small portion of the water discharged may be raised to a greater height than that of the original device which produced the pressure. This contrivance has been called a hydraulic ram, and by its help an increase of pressure may be obtained at the expense of a certain quantity of water.

Notwithstanding electricity is not a fluid like water, it behaves, however, also in this case, in a similar manner. At the moment a uniform electric current is suddenly interrupted, a current of greater electromotive force (more pressure) is developed in the conductor, and will show itself as a spark at the break of contact, and as a shock through the body when the hands have hold of the wires when the contact is broken. It is a very old observation, and one of the earliest reminiscences of my boyhood (about 1823), that the current from, say 30 pairs of plates, scarcely perceptible when closing the circuit through the body, will give a quite sensible shock when breaking the circuit. Such currents have been called extra currents, and they are, of course, stronger in proportion to the strength of the original current, and still further increased by inductive influences, when there are coils in the circuit.

As it may be desirable for special purposes to eliminate these currents, an apparatus has been invented, which operates like a very sensitive relay, and then leads these extra currents off, or shunts them into another channel after the manner of lightning protectors. Such an apparatus was on the table during the experiments with the dog, and appeared to be intended to protect the animal against the effect of these extra currents. I must confess not to see why this was done, except for the purpose to annul the most powerful and dangerous item connected with the direct current. Therefore I consider the addition of this contrivance improper if we wish to arrive at the truth in regard to the comparative danger of the alternate and direct currents, because in case of accidental exposure of person there is rarely such a safeguard inclosed in the circuit, and the person struck by the current must submit to the extra current as well.

In order to arrive at the truth of the matter we must also, above all, be able to make definite statements about the voltage and the volume of current, based on precise measurements, which, in regard to the alternate currents, could not be furnished, but was only guessed at.

Finally, as far as I can judge about the matter, guided by the present state of knowledge (especially in regard to some, in many respects, still unexplained peculiarities in the behaviour of alternate currents), I should say that their true voltage must be quite high. They do not behave like currents of low voltage (say 300 or 400), as they can be, and practically are, conducted over much longer lines and thinner wires than is the case with the direct currents of, say, 1,000 volts. This fact is acknowledged as well by the enemies as by the friends of the alternating current system. Experiments made by converters, especially with a new and very peculiar one of my own construction, appear also to show that there must be considerable voltage in the alternate currents so as to account for the generation of the enormous volume of current into which the electromotive force is converted; in other words, for the thousands of ampères obtained by the conversion of the volts of the primary current, and for which ampères there is no ammeter yet to measure them, but their volume shows itself in the capacity to perform operations requiring the highest temperatures, such as welding iron, fusing the most refractory substances, &c.

Surely no dynamo could be constructed directly generating a current of such volume, as it would destroy itself by the heat of its own current; it is only by alternating current machines of high voltage, assisted by proper converters, that currents of such great volume and high caloric capacity can be obtained.

After all, we must not lose sight of the main object of this controversy, which is the comparitive danger lurking in the two systems. When it is found by experience that actually the danger of being killed is greater by the use of alternate currents, it does not make any difference if the voltage which does the killing is 300 or 1,300.

When, however, we consider that statistics show that the loss of life by the direct current system is continually increasing in proportion that the system is extending, the probabilities appear to be that both systems will furnish their equal quota of victims, and the labour of comparing the relative danger of both systems, and especially the discussion about their comparative voltage, appears to become an idle occupation.

In brief discussion on this paper, Dr. O. A. Moses, referring to a letter of ex-Gov. A. B. Cornell published in the daily papers, advocating the prohibition of high tension currents, offered the following resolutions, which were unanimously adopted:—

"*Whereas*, The National Electric Light Association read with some uneasiness the persistent effort of rival interests to educate the public to a distrust of high potential electric currents, and

"*Whereas*, They would protest with all energy against recent efforts to pit against each other continuous and alternating current systems of electrical distribution, which efforts have as their covert purpose the destruction of the harmony of action now existing between them, and

"*Whereas*, This association for mutual protection recognises distinctly the danger of unfair legislation such as has destroyed the electric lighting interest of Great Britain; therefore be it

"*Resolved*, That the National Electric Light Association emphatically declares that it is beyond doubt possible to produce and distribute high tension currents for public use without any more danger or difficulty than attends the distribution of gas and water in our dwellings; and further be it

"*Resolved*, That they would impress all legislators with the fact that success in the electric light and power busuness is based chiefly upon economy in the size of conductors, and that to utilise this fact high tension currents were adopted in the first stages of development of electric lighting and have been used ever since and will continue to be used; and further, be it

"*Resolved*, That it is our conviction that there is no difference in the danger attending the use of continuous or alternating currents, and that both may be so transformed before being used as to render them perfectly harmless and tractable means of distributing electric power for use in cities."

ELECTRICAL CURRENTS CAUSED BY THE PULSATIONS OF THE HUMAN HEART.

THE Medical Session at the Hospital Schools commenced on Monday. At St. Mary's Hospital Dr. Augustus Waller delivered the opening lecture. After a few introductory remarks in allusion to the usual scope aad purpose of opening addresses, the lecturer said that he should take this occasion to give the results of a physiological research which had occupied him during the last four years, and described how he had been led to discover electrical currents caused by the pulsation of the human heart. Dr. Waller's description was as follows:—It is a well-known fact that every beat of the heart is accompanied by an electrical disturbance. The nature of this disturbance has, moreover, been studied and understood with the assistance of cold-blooded animals, and in this laboratory, in particular, an investigation was carried out to learn whether or no warm-blooded animals manifest similar electrical disturbances. These I shall not now enter upon, and will only make the passing remark that, while to all appearance the electrical disturbances are similar in the two classes of animals, they are not identically so; they seem to indicate that the contraction which at each beat of the cold-blooded heart runs down from the base to the apex, runs in the opposite direction in the warm-blooded heart. But this is only by the way, and I make no attempt to explain. It is to the next step that I invite your attention to-day. Led on from thought to thought, it occurred to me that it should be possible to get evidence of electrical action on man by connecting, not the heart itself, which is obviously impossible, but parts of the surface of the body near the heart with a suitable instrument. Having verified this supposition, the next step was to see whether or no the same evidence can be obtained by connecting the instrument with parts of the body at a distance from the heart with the hands or feet. The answer was, as you will see, satisfactory. Finally, I tried whether two people holding hands and connected with the instrument gave evidence of electrical shocks through each other, and I found they did. The only portion which I wish to explain in any detail is the second step in these experiments—viz., the analysis of the results which are obtained when a single individual, whether man, horse, or dog, is connected with the electrical indicator (capillary electrometer). When the two ends of an electrical indicator are in connection with any two points, A and B, of a living body, the index stands at zero if A and B are at the same electrical level (potential), and it does not move if the two points rise or fall together to an equal amount; if, on the other hand, the index moves up we know that A is lower than B; if down, that B is lower than A. Let us apply such an instrument to the human heart. This, which seems rather a bold proposition, is really a very simple and easy matter. We need simply dip the two hands into two basins of water which are in connection with our indicator when we shall see that the index moves up and down with each pulse. These movements of the index are due to the electrical changes which occur with every beat of the heart. Or we may dip a hand and a foot each into a basin of water with the same result, only it must be the right hand—the left will not do. This difference, apparently so curious and puzzling at first sight, which seemed unsymmetrical and irrational, is in reality most reasonable, and proved to be the the master key which threw open the meaning of every subsequent experiment. The difference depends on the unsymmetrical position of the human heart, to the fact that it is tilted to the left side. Allow me to return for a moment to the physical A B C of the subject, using the letters A and B as abbreviations for "apex" and "base" of the heart. If, with the contraction of the organ, these two parts, A and B, undergo any unequal or successive electrical change, the change will spread over the whole body, in accordance with known laws. I will say no more than that the form of the change will be represented by two systems of oval lines, with A and B as the centre of each system, which can be compared with the system of circular lines seen on a smooth pond soon after a stone has been thrown into it. If the electrical level falls at A it falls over an area which includes the left arm, the left leg, and the right leg. If the electrical level falls at B it falls over an area which includes the right arm and the head. Now, it is obvious that the two ends of the indicator must be connected with A and with B before it will indicate any difference between A and B; if both ends are connected with A, or both ends with B, nothing will be seen. This was precisely what we got when the left hand and either foot were connected with the instrument—it was silent; but if, instead of the left hand, we connected the right hand and either of the feet, we obtained sharp answers (movements of the index) with each beat of the heart. I might multiply instances, but will only just mention one: if the mouth and right hand are connected with the instrument its index does not move, but it does so as soon as the left hand is put in the place of the right one. But this evidence does not stand alone. Patients every now and then present themselves at hospitals with a transposition of the viscera, which are in such people situated like those of a normal person as they would be viewed in a mirror; the heart, among other organs, is reversed, and instead of pointing to the left points to the right. As regards the electrical relations which I was following out, they were precisely as expected. The left arm was in this case the exceptional limb, and formed an effective couple with any one of the other three limbs, but was ineffective in combination with the mouth. Let us hear one more witness. The heart of a quadruped (dog, or cat, or rabbit, or horse) is placed far more symmetrically than in man; it is very nearly in the middle line, so that the changes of electrical level, whose *foci* are at A and B, spread straight up and down the body, not obliquely as in man. The upper half of the body is under the influence emanating from B, the base of the heart; the lower half under that emanating from A, the apex of the heart. Unlike what occurs in man the two front paws or feet, coupled with an indicator, are silent, while either front foot taken in conjunction with either hind foot gives us the now familiar answer. These are the principal facts. What can we learn from them with regard to the normal action of the heart. I must be content with simply stating the answers. The fact that each beat of the heart gives an electrical change beginning at one end of the organ and ending at the other proves that the contraction does not occur throughout the mass of the heart at one and the same instant of time. If the two points, A and B, rose and fell together, there would be no alteration of the index. The movements of the index show that there is a fall of A at the beginning of the contraction and a fall of B at the end of the contraction. One of the most fundamental and certain facts in physiology is that the active state of a living tissue is marked by a fall of electrical level; in other words, an electrical depression is the best, most certain, and most delicate physical sign of physiological action. It proves the fact that living tissue is in excitement just as certainly as a dog's bark proves that a living dog is in excitement. In all the cases which we have considered A has barked first; B has barked last. The physiological significance of these events is that contraction commences at the apex and terminates at the base. We have here the answer, and more than the answer, to a question which has often been asked but never settled—viz., does the heart (*i.e.*, the ventricles) beat simultaneously in every portion, or does the contraction take place progressively as a state of action traversing the whole mass from a beginning to an end? The answer is a distinct affirmation of the second alternative, to the exclusion of the first, with the additional and unexpected rider that the contraction begins at the apex and terminates at the base. I have tried to sketch out in language as little technical as might be the scope and outcome of a physiological research. I fear—nay, I am sure—that to many of my audience it has been dry and tedious, yet I cannot but hope that some of you have been interested to hear, by an actual example, what kind of work goes on inside a physiological laboratory. I hope, moreover, that it may go a little way towards correcting that most unfortunate and mischievous error that the laboratory is a chamber of horrors. In conclusion, the lecturer referred to the immediate interests of medical students, dwelling upon the importance of combining fact-work in the laboratory and hospital wards with book-work in the study and lecture room.

NEW PATENTS—1888.

13629. "Improvements in brush holders for dynamo-electric machines." W. H. Scott. Dated September 21.

13643. "Improvements in circuit making and interrupting devices for electric bells and other purposes." W. Snelgrove. Dated September 21.

13653. "An electrical automatic money call box." E. R. Widdicombe. Dated September 21.

13687. "Improvements in and connected with electrical furnaces." R. E. B. Crompton. Dated September 22.

13713. "Non-magnetisable balance for watches and chronometers." D. Buckney. Dated September 22.

13717. "Means for automatically feeding electric batteries with induction liquid." S. Eloesser. Dated September 22.

13720. "Improvements in the construction of lightning conductors for the protection of buildings." S. A. Varley and G. Driver. Dated September 22.

13725. "Improved means or devices for making electrical contacts in coupling up electric conductors." T. M. Sargeant and O. R. Swete. Dated September 22.

13778. "Improvements in telephone central station apparatus." T. N. Vail and J. A. Seely. Dated September 24. (*Complete.*) [Date applied for under Section 103 of the Patents, &c., Act, 1883, February 25, 1888.]

13779. "Improvements in telephone sub-station apparatus." T. N. Vail and J. A. Seely. Dated September 24. (*Complete.*) [Date applied for under Section 103 of the Patents, &c., Act, 1883, February 25, 1888.]

13825. "Improvements relating to electrical conductors and insulating conduits therefor." H. H. Lake. (Communicated by R. F. Silliman, United States.) Dated September 25. (*Complete.*)

13937. "Improvements in transmitters for telegraphic purposes." P. Neil. Dated September 27.

ABSTRACTS OF PUBLISHED SPECIFICATIONS, 1887.

10131. "Improvements in meters or apparatus for measuring electricity." G. Forbes. Dated July 19. 8d. Is founded upon the fact that an electric current passing through a conductor of electricity generates heat, which heat is carried away from the conductor by the air in contact with it, thus forming convection currents in the air. The claims are 6 in number.

10157. "Improvements in and connected with apparatus for transmitting and receiving electrical signals." T. A. Garrett. Dated July 20. 8d. Claims:—1. Electrical signal transmitting instruments wherein there are in the presence of magnetic poles and of the ends of stationary iron cores coiled with wire armatures or pieces of iron attached to springs, which when caused to vibrate produce electrical vibrations in the wire coils and in the line with which these coils are connected, substantially as described. 2. Electrical signal receiving instruments wherein pieces of iron attached to springs are made to vibrate in the presence of magnetic poles and of the ends of stationary iron cores coiled with wire, when electrical variations are transmitted by the line through these coils, substantially as described.

10611. "Improvements in electrical batteries." R. P. Milburn. Dated August 2. 6d. Claims:—1. The use in a battery of bichromate of potash, sulphuric acid and water in combination and sufficiently amalgamated, on the one pole, and nitric acid at the other pole. 2. The whole arrangement of battery as described. 3. The whole arrangement of battery as described in conjunction with the liquids used.

10833. "Improvements relating to electric lamps." F. R. Boardman. Dated August 6. 8d. One of the carbons is made tubular to admit a small or pencil carbon; and this is pushed through the same and through a corresponding groove or canal in a marble block, until it comes into contact with the opposite or solid carbon electrode. This establishes electrical connection between the carbon poles, and the pencil carbon being withdrawn, the arc is struck, and the block becomes incandescent and luminous. Also relates to an apparatus by means of which the pencil carbon is made to establish electrical contact between the carbon electrodes and to break the same automatically; either by the action of the main current, or by a shunt current. The claims are 4 in number.

10838. "Improvements in electrical switches." F. L. Rawson and W. White. Dated August 6. 8d. Claims:—1. The combination in an electric switch of a switch arm consisting of spring plates of metal pressed upon contact pieces by a spring washer on its spindle and pressed by springs in the plane of its rotation so as to cause the switch to break contact sharply. 2. In an electric switch the application of springs composed of pieces of spring wire to effect the quick breaking of the electrical current, substantially as described. 3. In an electric switch the arrangement of a fusible cut-out upon its base, substantially as described. 4. In electric double-break switches the combination of a switch arm consisting of spring plates of metal bent over at either end so as to press upon contact pieces, with a spring or springs pressing in the plane of its rotation so as to cause the switch to break contact sharply. 5. In an electric switch, the combination with the switch arm of a spring washer situated under the base of the switch and reacting so as to produce elastic pressure between the switch arm and the contact pieces.

10952. "Improvements in or connected with dynamo-electric machines and electro-motors." L. Anspach and L. Gerard. Dated August 10. 8d. The armature is wound and connected in such a manner that the tension at which the commutators work is only a fraction of the total tension, and that the drawbacks resulting from great differences of tension between adjoining segments are thus avoided. The armature carries on the same magnetic drum or cylinder, two or more commutators mounted on the shaft of the dynamo machine. The commutators are joined together in series (the negative brushes of the first commutator being directly connected with the positive brushes of the second, and so on, while the positive brushes of the first and the negative brushes of the last commutator are connected with the line or external circuit). The claims are 3 in number.

11252. "Improvements in secondary batteries or electrical accumulators." H. MOWER. Dated August 17. 8d. The inventor makes the support or lead portion of the plate which is to form the support for the active material in the form of a grid or openwork support. It may be either made of drawn metal or of cast metal; for instance, it may be made of a strip or strips bent alternately in one and the other direction, so as to form parallel bars on either side of a suspending piece also made from the same strip or strips. The bent over ends of the support thus formed are joined by a portion of the same strip or by a separate piece. The same general figure may also be obtained by casting. The spaces between the supporting strips, formed as described, are filled in with the active material, or material to be rendered active, and this is by preference mixed with hair, feathers, or other animal matter of like character, to give it coherence. One or more of the spaces between the strips, however, is not filled in with the said material, but is left open. The claims are 3 in number.

11901. "Improvements in lamp holders or sockets for incandescence electric lamps." R. A. SCOTT. Dated September 2. 8d. The inventor employs a holder or socket consisting of a metal cylinder for attaching to the fitting, containing a spiral spring and a base block, which latter is either fixed or free to revolve, and constructed of slate or other insulating material, and having two small holes therein for the circuit wires to pass through, which wires being sufficiently bared of insulation, are formed into hooks by pliers, and the lamp being hooked on, contact is maintained by means of a lateral thrust imparted to the lamp through an outer tube of brass carrying prongs or elastic projections, which prongs or projections form a steadying seat for the body of the lamp; the lateral thrust before mentioned is imparted to the outer tube carrying the prongs or elastic projections by the spiral spring contained in the metal cylinder, the force being transmitted through slots in the cylinder. There is 1 claim.

12015. "Improvements in incandescent electric lamps." W. LONHOLDT. Dated September 5. 8d. Consists in composing the filament casing of two pieces so combined one with the other as to permit of being separated and put together again, so that the lamp can as often as may be desired be provided with a new incandescent filament. There is 1 claim.

12111. "Improvements in electric fuses for mines." W. P. THOMPSON. (A communication from abroad by E. Dumas, of Paris.) Dated September 7. 8d. Claims:—1. In electric fuses for mines, the coating of the extremities of the conducting wires and the platinum buckle connecting the same with an impervious varnish (preferably collodion) which explodes at a low temperature, the said varnish being preferably afterwards covered with a light coating of priming powder or other suitable explosive substance, the whole substantially as and for the purpose described. 2. In electric fuses for mines having the platinum buckle coated with an impervious varnish, the mode of attaching the said buckle by simply clinching it to the extremities of the conducting wires, as described and shown on the drawings. 3. In electric fuses the application to the finished fuse, whether provided or not with a detonator charged with fulminate of mercury, of a coating of collodion or its equivalent extending for a short distance along the insulated conductors, in the manner described and for the purpose specified.

12325. "Improvements in telephonic apparatus." W. FAIRWEATHER. (A communication from abroad by the Western Electric Company of Chicago.) Dated September 12. 8d. Has for object the provision of improved means whereby in a telephonic exchange system the extension of an existing subscriber's line to an additional subscriber may be effected without the use of a separate wire from the exchange, and whereby with the use of a single line wire between each subscriber and the exchange, ringing off or other signals may be communicated by any subscriber to the exchange operator without calling the other subscriber with whose instrument connection is established, or one subscriber may signal to another, after connection has been established through the exchange, without at the same time calling the exchange operator. The claims are 3 in number.

12683. "Improvements in the manufacture of spongy lead suitable for electrical accumulators." F. M. A. LAURENT-CELY. Dated September 19. 8d. Claims:—1. The process described of manufacturing spongy lead suitable for electrical accumulators, that is to say, by melting a lead compound, then running or casting it in conducting supports, or casting it into blocks which are subsequently set in conducting supports, or casting it into blocks which subsequently have conducting supports cast around them, and then reducing the lead compound to the state of spongy lead by means of hydrogen obtained by the decomposition of water by an electric current, substantially as set forth. 2. Manufacturing spongy lead suitable for electrical accumulators, as described.

12718. "Improvements in electrical speed indicators." J. TATE. Dated September 20. 8d. In a suitable casing attached to an electro-magnet the inventor mounts a ratchet wheel containing by preference the same number of teeth as strokes, or revolutions, the engine or other machine should make per minute, and operates the wheel one tooth each stroke or revolution by a pawl attached to a reciprocating bar. To some convenient portion of the said ratchet wheel he secures a pin, which comes in contact each revolution of the said wheel with a weighted arm on a stud, the said pin carrying the weighted arm around the stud until it arrives in a certain position, when it moves by gravity on the said stud about half a revolution, causing a spring to move and complete an electric circuit, the weighted arm being retained clear of the spring until again operated by the pin secured to the ratchet wheel. By completing the electric circuit in the manner described, the current is sent through the connecting wires to the dial portion of the apparatus in the office or other portion of the works, and therein operates a lever and pawl, which actuates a ratchet wheel every time the circuit is completed, thereby moving the fingers on the dial once every minute, so that the minute and hour fingers will indicate the time run and show the number of minutes the engine or other machine has gained or lost during the day or week. The claims are 2 in number.

CORRESPONDENCE.

"What is Electricity?"

Every man knows, or fancies he knows, what he means, most men think they know how to express what they mean, but it is allotted to very few to have the power to convey accurately to others what they mean. Your editorial note on p. 330 of your number of September 28th leads me to fear that I have sadly failed in the last point. In making the statement that electricity is certainly not energy you involuntarily affirm one of my contentions. It is the dogmatic assertion and not the fact itself that I object to. There are two things in question:—There is a something which we will call A that *is not* a form of energy. It is measured by coulombs. There is another thing which we will call B that *is* a form of energy. It is measured by watts. A coulomb per second is an ampère, as is also a watt per volt. A and B are thus related. A volt-coulomb is equivalent to a watt-second, each of which is clearly energy. A coulomb is therefore a factor of energy while a watt is energy. Now when you assert that electricity is not a form of energy you apply the term electricity to A; when I assert that electricity is a form of energy I apply the term to B. Hence we are speaking of different things. If A were called coulombism, and B alone electricity, or if A alone were called electricity and B wattism, or any other ism, confusion would cease; but as long as the term electricity is indiscriminately applied—

(1). To the science of electrical phenomena,
(2). To one factor of electrical energy,
(3). To the ether which fills all space,
(4). To a mysterious something that permeates the ether,
(5). To a form of matter purely imaginary and *sui generis*,
(6). To a something that is associated with each atom of matter,
(7). To a special fluid that has no real existence,
(8). To that form of energy which we call a current—*et id genus omne*,

so long will there be confusion and disagreement. I have given strong reasons why the term should be confined to that form that we manufacture and supply, that we buy and sell, that we measure and use, and that is understood by the public. The great step in advance that was chronicled at the British Association at Bath was the fact that electro-magnetic waves in the ether have been admitted and measured. There is nothing like the logic of measurement to dissipate error. I hope your readers will study Prof. Hicks's paper read at the same meeting. It opens quite another phase of the question. The material theory of electricity has now been exorcised to regions "deep as e'er plummet sounded," and the mechanical theory has come to the front. The true question is not "What is electricity?" but what is a coulomb? I do not attempt to answer the question, but I object to its being called electricity. I wish to restrict the use of the term electricity to the form of energy which has an objective existence.

W. H. Preece.

G.P.O., *October 2nd*, 1888.